SCIENCE.

FRIDAY, SEPTEMBER 28, 1883.

THE NATIONAL OBSERVATORY.

WE call the naval observatory at Washington 'national,' not because we would ignore its recognized official title, but because we wish to emphasize the facts, so often lost sight of, that it is the property of the nation, that it is the only observatory of the first class which the nation possesses, and that its operations should be equally available for every department of the government. Such an institution is a national one, by whatever name it may be called; and the question of its direction and supervision is one of interest to every government office having need of such astronomical observations as can be made only at a fixed observatory. The general principle that it should be under purely scientific control is one that has generally been conceded in the abstract, but has not always been acted upon. Sears Cook Walker, who, thirty-five years ago, was perhaps the most eminent astronomer of America, propounded this principle in a published letter; but Maury was then near the zenith of his power, and little notice was taken of the opinion of the subordinate. From that time to this, the superintendency has remained in the hands of line-officers of the navy. The officers of our navy are of too high a character, and have too much self-respect, to pretend to a knowledge which they do not possess: we may therefore inquire how it happens that they claim the exclusive direction of an establishment most of whose operations are outside the line of their professional qualifications. Secretary Chandler has never given official utterance to his views; but he is understood to have said that he did not feel authorized to deviate from a precedent which had been sanctioned by forty years of usage. Precedent is, in one form or another, the basis of the principal argument on which the present system is sustained: we shall therefore inquire whether it has any real validity.

In order that such a supposed precedent may afford any sound reason for its continuance, the system must have resulted from the matured judgment of his predecessors, whose acts the new secretary followed. Unless this was the case, unless he was doing what they would have done under the same circumstances, the argument could have had no legitimate weight. Now, if one looks more closely at the case, he will see that there is a great deal of precedent on the other side. With one exception, not a superintendent had ever been appointed before his time who was not a professional astronomer, or had not some standing in the scientific world. Maury, Gilliss, Davis, and Rodgers were all recognized as having, in some form, qualifications arising from eminence in science or from a familiarity with scientific affairs; and it was this consideration which prompted their selection, and not merely the fact that they were naval officers. We might therefore claim that Secretary Chandler himself had deviated from precedent in appointing superintendents on the sole ground of naval rank. Indeed, we believe that Secretary Chandler was the first who ever gave any real hearing to what the astronomers of the country had to say on the subject. On all previous occasions, vacancies in the superintendency had been filled so quickly, that they never had had time to give an organized expression to their views at the critical moment, even supposing they had been disposed to find fault with the selection, which certainly was not always the case. A plaintiff whose suit had been postponed from time to time for forty years might well feel dissatisfied, if, when finally heard, the decision of the judge should be, that the defendant had remained so long in possession, that he must now keep possession, no matter what the merits of the case. It should not be forgotten that the theory that the

observatory needs nothing but an administrative officer, whose sole duty it shall be to take charge of the building and grounds, preserve order, and conduct the correspondence, leaving the scientific work to the professors and lieutenants, was never heard of, except when no other argument was available, and is now not likely to be supported even by the line-officers themselves.

The tersest form in which the case is put by these officers is this : " The system has been tried for forty years, and has worked well ; let us leave well-enough alone." But has there been any system? Certainly not, unless a total absence of system can be called a system. And in what way has it worked well? This depends on the standard by which we measure it. We may admit that in the eyes of the conservative public every thing which does not lead to utter destruction, or against which nothing is heard, is looked upon as working well. We once heard a popular superintendent highly praised, because, having the professors completely in his power, he did not embarrass them by vexatious interference, but had the forbearance to let them go on with their work without hindrance. Last spring, when the question had given rise to a lively discussion among scientific men generally, one of the most eminent foreign astronomers who has landed on our shores paid us a visit. He was, of course, restrained from any public expression of opinion on the subject, but could respond frankly to all inquiries. When asked for his views, he said in substance that individual astronomers had done important works, and made great discoveries at the naval observatory. But, he added, when we look further, and inquire what the observatory itself has done by organized work, we find a great want. There has been no unity, no continuous plan of work, and few of the results which might have been gained by organized action. He might have stated the case yet more strongly. The published observations of the thirty-five years are of every possible character, from the refined discussions of the accomplished astronomer to the vain efforts of the tyro working in the dark, and the confused records of careless men who did not know what to do, and cared for nothing except to draw their pay, — all put in without discrimination. The astronomer of the future who shall try to make use of the results will be surprised by the kaleidoscopic character of the impression made upon him as he turns from volume to volume. Here a new series of observations suddenly begins. He will follow them through a few months or a few years, and find them as suddenly broken off, right in the middle, perhaps, and just when they might have led to some useful result. New systems of observation and new methods of calculation will be found coming in from time to time without any apparent reason. Every effort he may make to discover a method in the madness will be vain. To find an explanation, he will have to inquire into the *personnel* of the observers. By careful research he will then find, as a curious coincidence, that, when these changes occurred, some observer had died or left the observatory, or there had been a change of observers at the instruments. And this is the so-called 'system,' to the perpetuation of which the country is asked to dedicate the new observatory, to be built at a cost of half a million dollars.

The attitude of the naval officers, under these circumstances, is of much interest, because it depends very largely on them to determine whether this confusion shall continue indefinitely, or whether some permanent plan of work shall be adopted. If the indications of their views and intentions which have reached us since the discussion began are correctly interpreted, they have resolved on a course which cannot but prove equally disastrous to naval and national science. Common report credits them with a determination to 'hold the fort' at all hazards, and to vigorously contest every effort that may be made to place the observatory under scientific control. There are even indications that the dismissal of some or all the civilian astronomers is desired, in order that none but naval officers may be left to do the work.

Such a prospect naturally leads us to consider the relations of the navy to science. Scientific organizations have shown on every occasion their high appreciation of the efforts of naval officers to secure a scientific training for themselves, and to advance knowledge by their own efforts. Every thing they have done has met with generous recognition from their civilian co-laborers, and they are received upon terms of perfect equality in every enterprise in which they have taken part. There is no scientific position which would be denied them on the ground that they were naval officers, and therefore to be regarded as inferiors. To maintain this cordial relationship, nothing more is necessary than that the officers should admit the equality, and make no claims except those which are founded upon merit. When they begin to claim precedence and control on the ground of naval rank, they assume a position in which they will meet with the combined opposition of their scientific co-laborers, and render all co-operation impossible.

The application of these considerations to the present case is very simple. Naval officers will not find, in scientific quarters, the slightest opposition to their doing any work at the observatory which will either advance science, or lead to their own professional improvement. It is, indeed, a mooted question, whether the work can really be well performed by any but a permanent staff of trained assistants, and it must be admitted that the observations made by naval officers in the early years of the establishment were not a success. But the officers may justly claim that what they did then is no test of what they can do now, when a better training has been secured, and a scientific spirit has been infused into the service. There is no such question raised on the scientific side as, Shall you or shall we do the work? Shall you or shall we superintend it? What is, then, the ground taken by the general scientific sentiment of the country? Of course, in answering a question of this kind, differences of individual views will be found, and no answer can be given which all will accept without modification. But we are persuaded that there will be no difficulty in reaching some conclusions which will correctly represent the average common sense of the great mass of those who are interested in the subject. We state them as follows: —

Give the naval officers every possible chance, and let them do every thing which they shall prove themselves able to do. Let the superintendent be the man, who, in the opinion of the astronomers of the country, is best fitted for the place, whether naval officer or civilian.

But let the questions, what shall the observatory do, how shall it be done, and is what is done good, be decided exclusively by the highest scientific authority, acting, not privately, and upon the motion of the superintendent, but officially, with the weight and responsibility of legal appointment. Let this authority represent, not merely the navy department or naval science, but the science of the whole country, and let the superintendent, whoever he may be, be responsible for executing its decisions. The shape it would naturally take would be that of a board of control, composed of the leading astronomers of the country.

We state these points, not as forming a definite plan, or even laying a basis for such a plan, but only as indicating the spirit in which we hold that the case should be considered by the two parties. What we ask is as much for the intellectual benefit of the navy itself as for the good of science, and we earnestly hope that naval officers will meet our views in the spirit in which they are put forth.

THE NATIONAL RAILWAY EXPOSITION.[1] — V.

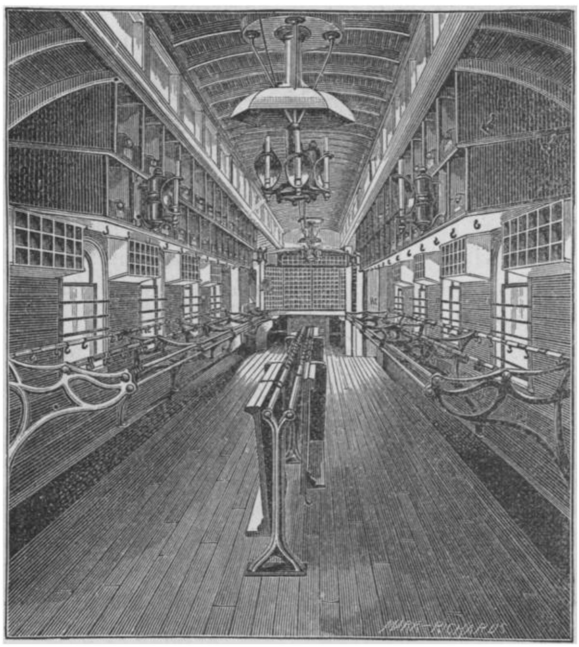
THE postal-car shown by the Harrison postal-bag rack company of Fond du Lac, Wis., appears to be conveniently arranged, and possesses many ingenious but simple devices for facilitating the conveyance and sorting of letters and newspapers. The sorting-tables are not fixed, but are hinged by means of hooks on movable stanchions; and each table, measuring about forty-two inches by eighteen inches, can be detached and stowed away, so that any num-

[1] Concluded from No. 26.

ber can be utilized, and the remaining space left clear. The mail matter can also be sorted directly into bags, which are hung open mouthed, at their four corners, on cast-iron brackets, and these can also be folded out of the way when not required. The letter-boxes are provided with clips, into which labels can be inserted, showing the destination of the letters sorted into each particular box.

The Pullman palace-car company had a very large exhibit of sleeping and dining cars, including an emigrant sleeping-car, which will doubtless prove a great luxury to settlers journeying to the far west. The berths are arranged as in an ordinary sleeping-car, but consist merely of slats of ash, the bedding and mattress (if any) being provided by the emigrants themselves.

A new style of sleeping-car, the second of its kind ever built, was shown by the Paige sleeping-car company. The top berth does not fold up against the roof of the car, but is a species of rectangular hammock, hung at the ends from partitions between the sections. These partitions, in the day-time, are lowered into a space between the backs of the seats. The lower berth is not made on the seat, but on a similar canvas hammock.



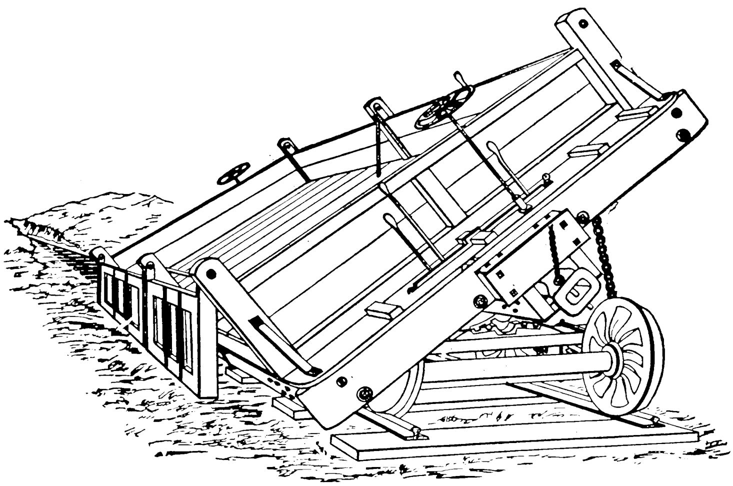
POSTAL-CAR RACKS.

A screw lever dump-car on Van Wormer's patent is shown by the U. S. car company of Boston, Mass. The centre support on the trucks is a species of ball-and-socket joint, combined with segments of two-toothed wheels, — one segment being bolted to the top of the truck-bolster, and the other to the under side of the bottom framing of the car; the effect being, that, when the car is tipped, it rolls on the trucks, the fulcrum on which it rolls being brought directly under the centre of gravity of the car and its load, which, of course, shifts as the car is tipped. When the load is dumped, the position of the centre of gravity tends to restore the car to its normal position: the arrangement, therefore, assists the man in charge both in dumping, and in restoring the car to its running position. The rockers, with the central ball and socket and segmental cogs or teeth on either side, are shown in our illustration. When the car is to be dumped, the side-supports are withdrawn by means of levers on the end-platforms, and the car is tipped to either side by means of a worm actuated by a hand-wheel. As the bottom of the car is solid, it can be made stronger than a hopper-bottom car, and can be used for freight, which requires a flat floor, and cannot be loaded in a hopper-bottom car. It is stated that one man can unload forty thousand pounds of coal, sand, ballast, or iron ore, in two minutes by means of a dump-car, two hours being required to shovel out the same load.

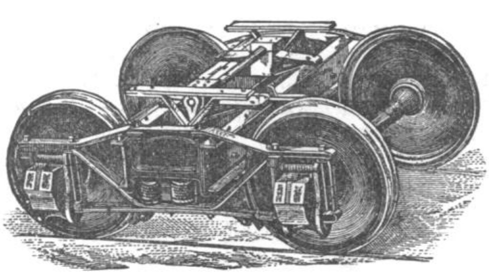
The Suspension car-truck manufacturing company of New York exhibited several trucks made on their principle, suited for freight, passenger, and horse cars, and showed a model truck which traversed an abnormally rough piece of track with a very smooth and easy motion. The car is connected to the truck by means of links, which swing in a vertical plane parallel with the track, instead of at right angles to it, as in the swing-beam truck; while the axle-boxes are connected to the trucks by means of links, which permit independent side-motion to each axle. The normal position of all the links is vertical, and they become inclined as the truck enters a curve, and therefore tend to restore it to a central position when the truck enters a piece of straight track again.

The principle of the truck is entirely novel, and, though really simple, is best understood by a few minutes' examination of a model. Two brackets, resembling the letter A reversed, are attached to the under side of the car. At the

apex (now the lowest point) of the A are attached vertical links, the other end of which are attached to the truck at *B*, *B*, in our diagram. As the truck enters a curve, one of these links becomes inclined forward, and the other backward. As the wheel *C* strikes against the outer rail of the curve, it is thrown towards the inside of the curve (assuming the position shown by the dotted lines in the figure), and the suspension-links force that side of the truck forward, while the wheel *D* comes backwards; and therefore the action of the links tends to make the axles radiate to the curve. No centre-pin is used; and therefore, when a car is heavily bumped in switching, it merely swings backward on the links until they become sufficiently inclined to drag the truck after the car. It should be noted that the pin *E*, which connects the links to the truck, is a loose fit in the links, and therefore allows of the necessary radial motion. The top ends of the links, being attached to the truck, are always approximately a fixed distance above the rails; and therefore, when they are inclined, the car itself is lifted, and the weight of the car, hence, tends constantly to keep the links vertical, and maintain the truck in its normal position, with the axles



DUMP-CAR.



SUSPENSION CAR-TRUCK.

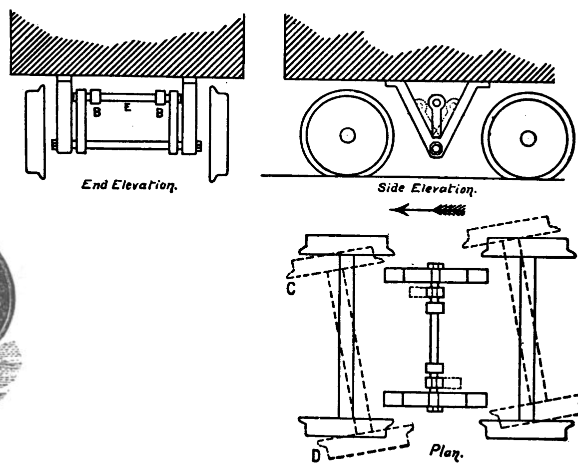
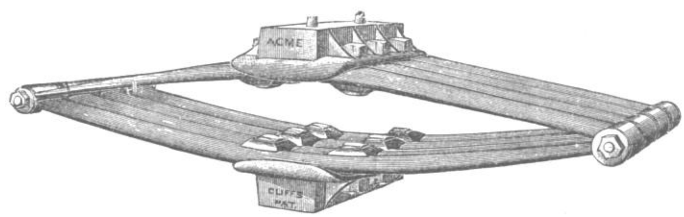


DIAGRAM SHOWING ACTION OF SUSPENSION CAR-TRUCK.

at right angles to the axis of the car, so that it runs steadily on a straight line. The truck appears to be very highly thought of by the master car-builders, whose convention was held in Chicago during the exposition; and it is possible that it may come into extended use, the experience of the Boston and Albany, Connecticut River, and other roads which have used it, being strongly in its favor.

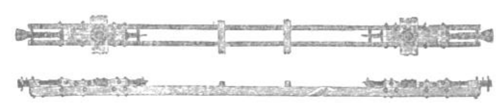
The Cliff and Righter company of Oswego, N.Y., exhibited a car-spring which gives an equal amount of elasticity, with a less amount of metal than the ordinary elliptic spring. Each half-spring consists of a solid steel bar of oval section, properly tapered towards the ends. Springs as usually made, of four, five, or more plates, resemble a set of somewhat elastic girders, the depth of each of which is the thickness of the plate; and the strength of a spring is the sum of the strength of each individual plate or girder, modified by the fric-

tion between the plates. It is obvious, that, were the plates to firmly adhere together, the strength of the spring would either be very largely increased, or the same strength might be attained by the use of a less number of plates; and the latter course has been carried to its limit by the patentee of the Cliff spring. A spring made of one plate must be of good steel, as, when loaded, the difference in the alteration of the lengths of its upper and lower surfaces is considerable, demanding a highly elastic steel. In the spring we illustrate, four springs are arranged side by side, — a plan which unites the advantages of a plate spring and a solid spring. Should one spring break, the other three will probably carry the load, while four springs side by side weigh no more than a spring of the same total strength, composed of a single bar of the same thickness, but of four times the width. A set of these springs for a passenger-car weighs nine hundred and twenty-eight pounds, while a set of the Pennsylvania railroad standard springs for the same purpose weighs sixteen hundred and thirty-two pounds, a difference of seven hundred and four pounds in favor of the solid spring. These springs have been lately introduced, and are being tried on the Boston and Albany and other railways. The difficulties of tempering and making a spring of one solid bar are considerable; but it is to be hoped they may be surmounted, as the weight of cars is a serious evil, "which has increased, is increasing, and ought to be diminished."



ELLIPTIC CAR-SPRING.

Mr. S. P. Tallman of New York exhibits a safety-drawbar for cars. Two pieces of timber are bolted between the middle sills of a car, and others are bolted to the under side of these timbers and the middle sills, forming a solid mass of timber, which receives both the buffing and drawing strains, the drawbar running through the timber, and being provided with springs at both ends. The spring nearest the draw-head takes the buffing-strain, and the spring at the end of the drawbar serves as a draw-spring. The disposition of the timbers enables them to be secured by more than the usual number of bolts, and the arrangement appears to be strong and simple, and not so liable to failure as the ordinary draught timber.


DRAWBAR.

Numerous refrigerator-cars were exhibited; and doubtless improvements will be much facilitated by the opportunities thus given to secure information, though it is to be regretted that the management of the exposition did not take steps to secure an efficient competitive trial of the cars under practical conditions. Beer, fruit, vegetables, etc., might have been placed in the cars, and locked up for a few days, when a careful examination of the contents would have given some indication of the relative merits of the cars.

The use of continuous brakes on passenger-trains has been found to be so advantageous, that their adoption on freight-trains is merely a question of time. Several forms of continuous brakes, applicable to freight-trains, were exhibited; the Westinghouse brake company showing a cheaper form of their well-known automatic brake, the reservoir being made of cast iron, and bolted to the cylinder. The triple valve, however, and other parts, differ little, except in size, from the brake used on passenger equipment. A cheaper form of brake, which requires no special pump or other fittings on the engine, or even a continuous brake connection through the train, is operated by the action of the ordinary hand-brake on the tender. The consequent compression of the draw-heads in the train is made by the peculiar mechanism of this brake-gear to apply the brake-shoes to the wheels of the cars.

This form of brake is peculiarly applicable to freight-service, as it allows of cars not fitted with the brake being run in the train without interfering with the use of the brake on the cars equipped. This class of brake can hardly be termed 'automatic' in the fullest sense of the term, insomuch as it does not work, should the train part in two. On the other hand, failure on any one car cannot impair the efficiency of the brake on the rest of the train.

The American brake company of St. Louis, Mo., exhibited full-sized working-models of a brake of this class.

Between the floor-sills, and at the inner end of the drawbar, is hung a bell-crank lever, *B*, which carries in one of its jawed ends the push-bar *A*, and, in the other, double-pull rods carrying a spiral spring transmitting the strain to bell-crank levers, *D*,*D*, suspended from the sills by hangers, *C*,*C*. The bell-cranks *D*,*D*, are connected to the brake-beams; and consequently compression on the draw-head acting on the lever *A* causes the brake-shoes to be pressed on the wheels, the amount of pressure being regulated by its transmission through the spiral spring. But, since a brake simply made as above described would not admit of a train being backed, a device is attached which removes the objection, and, further, only allows the brake to be applied when the car is moving at a speed above six miles per hour.

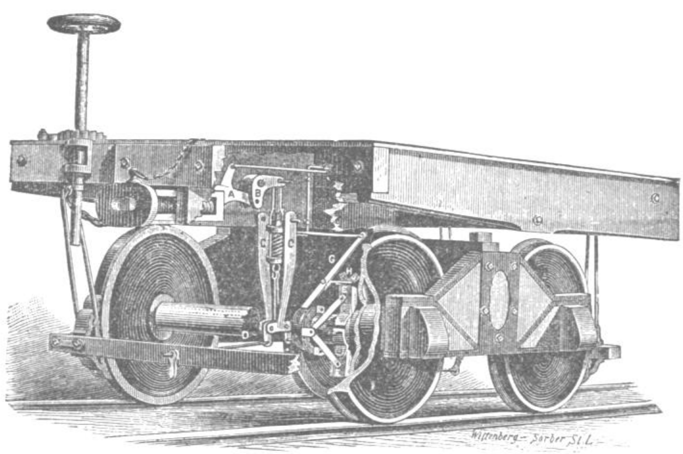
The push-bar *A* can only come in possible contact with the draw-head by the centrifugal force of governor-balls attached to the axle. These balls, *E*,*E*, are attached by means of links to a movable disk, *F*, encircling the axle. One end of a lever, *G*, bears against the disk, and the other end is connected by means of rods, etc., to the push-piece *A*. When the car is running at speed, the governor-balls draw the disk towards them, leaving the lever *G* free to follow it, and permitting the push-bar *A* to drop behind the draw-bar, when the brake is ready for action, going on directly the draw-gear is put in compression. When the speed falls below six miles an hour, the centrifugal force of the governor-weights becomes so feeble, that a spring (not shown in the illustration) restores the disk to its former position, lifting the push-piece *A* clear of the draw-head.

The brakes can be released at any time by the engineer putting on steam, and giving a pull to his train; and the train can be backed from a state of rest without the brakes going on, the push-piece *A* lying on the draw-head, but being unable to fall behind it.

The engineer can apply the brake, when pushing the train, by momentarily applying the brake on the engine or tender, thereby putting the draw-gear in tension, and letting the lever *A* fall behind the draw-head. When steam is again put on, the consequent compression applies the brake.

This brake has been in use for some time on the St. Louis and San Francisco and many other railroads, and appears to give very satisfactory results; the wear being very small, while the first cost is low enough to allow of its extensive application to freight-cars.

The brake exhibited by the Tallman automatic car-brake company of New York also acts by the compression of the draw-heads, which force together two friction-wheels, one of which is keyed on the axle, and the other is geared to a drum winding up the brake-chain. A ratchet-wheel, which can be shifted by hand, prevents the brake from acting when the train is backed.



AUTOMATIC BRAKE.

The Waldumer electric brake company of Chicago exhibited a working-model of a very promising form of continuous brake, which is just emerging from the experimental stage. The weak point of all continuous brakes has been the conveyance of the operating force — compressed air, vacuum, hydraulic power, etc. — along the length of a train, the pipes and couplings being generally expensive, and formed partly of perishable substances, while chains are unsatisfactory from every point of view. Many brakes that work well and promptly on a short train become slow and irregular in their action, when applied to a train of thirty or more vehicles. The instantaneous action of electricity, and the simplicity of the means used for its transmission, make it probable that an electric brake would be especially suited for long freight-trains. The

brake is automatic, the fracture of wire or draw-gear ringing bells on engine and caboose, and warning both engineer and conductor that the train has parted, each being then at liberty to apply the brake or not, on his portion of the train, as he may deem best. Owing to the system of circuiting, the brake may be out of order on one car without affecting the rest of the train.

The street-car starter and brake exhibited by Charles T. Brown & Co., Chicago, is an ingenious device for storing the momentum which is destroyed by the usual form of brake, and utilizing it for restarting the car. The motion is not checked by friction, but by the axle, which, through suitable gearing, winds up a spiral spring, the power of which is available to again put the car in motion. The mechanical details appear well worked out, and the car can be run in either direction, and stopped or started on either up or down grades. The heavy pull necessary to start a car is very severe on horses, and this invention would appear to be useful in saving much wear and tear of horse-flesh.

D. H. O'NEALE NEALE.

A HEARING OF BIRDS' EARS.[1]—I.

THE 'musical class' of vertebrates enjoy the sense of audition to a high degree. Otherwise birds would cease to sing. They are the only animals besides man whose emotions are habitually aroused, stimulated, and to some extent controlled, by the appreciation of harmonic vibrations of the atmosphere. Most birds express their sexual passions in song, sometimes of the most ravishing quality to human ears, as that of the nightingale, skylark, or blue-bird; and it cannot be supposed that they do not themselves experience the effect of music in an eminent degree of pleasurable mental perturbations. The capability of musical expression resides chiefly in the male sex; the receptive capacity of musical affections appears to be better developed in the female. There is, however, no anatomical difference in their ears. Quickness of ear is extraordinary in some birds, as those of the genus Mimus (mocking-birds), which correctly render any notes they may chance to hear, with greater readiness and accuracy than is usually within human compass; and it may be reasonably doubted whether any other animals than some of the world's greatest musical composers have a higher experience of acoustic possibilities than many birds possess.

Birds' ears have nevertheless a simple anatomical construction, in comparison with those of mammals. The auditory organ is decidedly of the reptilian type; and the arrangement of the parts is, on the whole, quite like that of reptiles. Thus, the cochlea, which in mammals makes from one and a half to five whorls (two and a half in man), is simply a strap-like prolongation from the vestibule, lacking modiolus, lamina spiralis, etc.; the stapes is the only perfected ossiculum auditûs; the incus is scarcely recognizable as such, and inseparable from the stapes; the malleus is immense, but outside the ear, furnishing the articulation of the lower jaw, of the zygomatic arch, and of the pterygo-palatal bar; the tympanic bone is represented at most by a few specks of ossification. There is ordinarily no external ear; the whole tympanic cavity is exposed on removal of the membrane, which lies very superficial; the eustachian tubes unite before opening into the pharynx; the periotic bone, constituting the otocrane or skull of the ear, is less compact and precise than the 'petrous portion' of the mammalian temporal bone, its three bony elements being more distinct; no mastoid portion is recognizable as such, but pneumatic cells of diploë are numberless, and there is direct passage of air from the ear into the hollow of the lower jaw; one of the semicircular canals invades the occipital bone. Other peculiarities will appear as we proceed with our description, in which comparisons will be chiefly made with the human ear.

Most birds have no external ear, in the sense of a fleshy conch or auricle. In bald-headed birds, the meatus externus appears as a roundish orifice at the lower back corner of the head, just above and behind the articulation of the lower jaw. In nearly all birds, the opening is hidden by an overlying packet of feathers, collectively termed the *auriculars* or ear-coverts, on simply raising and reflecting which the meatus is exposed. The auriculars are peculiarly modified feathers, having loosened barbs, doubtless to lessen interference with the passage of sound. In a few birds the border of the meatus develops a slight tegumentary fold, partially occluding the orifice. In various owls, as of the genera Strix, Aluco, Asio, Nyctala, but not even throughout this group of birds, an immense tegumentary *operculum*, or ear-cover, is developed, which flap shuts down upon the ear-opening like the lid

[1] Complementary to the article entitled 'The nature of the human temporal bone,' *Journal of otology*, January, 1882. Some portions of that article may perhaps be made clearer by the present one, especially those relating to the parts of a temporal bone as elements of mandibular and hyoidean arches. Figs. 1–4 are borrowed from Prof. W. K. Parker's admirable essay on the development of the fowl's skull, in Encycl. Brit., 9th ed., art. Birds; figs. 5–9 are from Prof. I. Ibsen's beautiful memoir, as cited in the text.

of a box. It hinges upon the anterior border of the meatus, and shuts backward. In some cases the operculum is about as long as the whole skull is deep, and half as wide as long — say, two inches long by an inch wide. On raising such an ear-flap and turning it forward, enormous external bony ear-parts, covered with integument, are displayed. Such expanse of the outer ear results from extension of occipital and squamosal bones into a thin shell bounding the meatus externus above, behind, and below. In the best-marked cases of the kind, especially in Nyctala, the parts are exaggerated unsymmetrically on right and left sides, and the whole cranium is distorted. This inflation of the cranium does not affect the inner ear-parts, or the essential organ of hearing. It should be added, in passing, that the so-called 'ears' of various owls, as the 'long-eared' owl, Asio otus, and 'short-eared' owl, Asio accipitrinus, are simply tufts of feathers on top of the head, over the eyes; these topknots having nothing whatever to do with the ears. Their proper name is plumicorns.

Aside from any such irregularities, the outer ear, or meatus auditorius externus, is a considerable, shallow, roundish depression, in the situation shown in fig. 1, where the reference line 5 crosses it, and where the cross-like object (stapes) marked *st* is seen lying in it. Its ordinary boundaries are, the enormous *malleus* or quadrate bone, *q*, in front; the expanded rim of the *squamosal*, *sq*, above; the *tympanic wing of the exoccipital* (a production of the lateral condylar plate of the occipital, *teo* in fig. 2), behind and below. A bone unknown in human anatomy, the *basi-temporal*, which floors the skull from ear to ear, underlying the basi-occipital and basi-sphenoid, also usually contributes to the inferior boundary of the meatus. On removing the quadrate (malleus), the general tympanic depression is seen to be more or less directly continuous with the alisphenoid, and so to conduct into the orbital cavity; the boundary of the meatus being best marked behind and below by the expansive thin-edged shell of the tympanic wing of the exoccipital. To the brim indicated is attached the membrana tympani; the ear-drum being thus from the configuration of the parts quite superficial, instead of being at the bottom of a long cylindrical tube, as in man. There is, in fact, in birds, no 'meatus auditorius externus,' in the sense of a special bony tube; some slight specks of ossification, when any, about the tympanic membrane itself, being all there is of a *tympanic bone* ('external auditory process' of human anatomy).

Such shallowness, openness, and superficiality of the parts, brings the cavity of the tympanum or middle ear into full view on removal of the tympanic membrane. On looking into this cavity, as may readily be done in clean, dry skulls of any size, many objects of interest may be studied without further dissection. We observe in the first place a large (inconstant) number of *pneumatic foramina* leading in various directions, conveying air from the middle ear-passage into the air-cells of bones of the skull, including the lower jaw. The most special of these is a neat gristly or bony air-tube into the lower mandible. The mouth of the eustachian tube is a large orifice at the lower anterior part of the cavity. This tube,

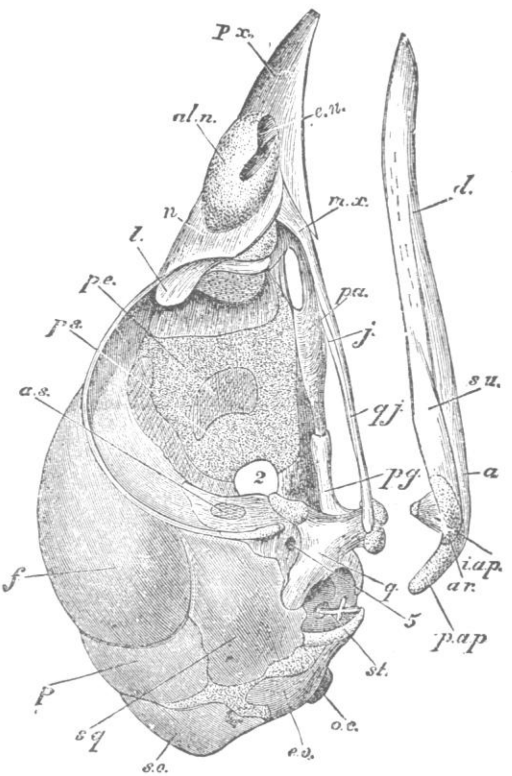


FIG. 1. — Ripe chick's skull in profile, × 3 diameters. (After Parker.) *px*, premaxillary; *aln*, ali-nasal cartilage; *en*, septo-nasal; *n*, nasal bone; *l*, lachrymal; *pe*, perpendicular plate of ethmoid; *ps*, presphenoidal region; *as*, alisphenoid; *f*, frontal; *p*, parietal; *sq*, squamosal; *so*, superoccipital; *eo*, exoccipital; *oc*, occipital condyle; *st*, the cross-like object, the stapes, whose foot fits *fenestra ovalis*; *q*, quadrate; *pg*, pterygoid; *qj*, quadrato-jugal; *j*, jugal; *pa*, palatine; *mx*, maxillary; *2*, *optic foramen*; *5*, *foramen ovale*, for inferior divisions of the fifth nerve. In the mandible: — *d*, dentary; *su*, surangular; *a*, angular; *ar*, articular; *iap*, internal angular process; *pap*, posterior angular process.

as usual, continues an air-passage to the pharynx, opening at the back of the hard palate by a median orifice in common with its fellow. In sizable skulls, as of a raven, hawk, or eagle, a bristle or even a wooden toothpick readily traverses the conduit which runs between the basisphenoid and the underlying basi-temporal. This whole passageway, from outer ear to tympanic cavity, and thence through eustachian tube to pharynx, represents the persistently patulous part of the first post-oral visceral cleft of the embryo, only occluded by the membrana tympani. Near the eustachian orifice are observed two definite openings. The anterior and superior of these is the fenestra ovalis, fitted, as usual, with the foot of the stapes, as seen in fig. 1, closed by membrane, which further occludes this opening into the *vestibular* cavity. The other is the fenestra rotunda, similarly leading into the *cochlear* cavity. The two are generally close together, separated merely by a bony bridge or bar. The former lies always in the obliterated suture between the proötic and opisthotic elements of the petrosal bone, the latter wholly in the opisthotic; both are thus as in man. Close examination at a point somewhere about the fenestra ovalis will discover a minute foramen, corresponding to the human 'stylo-mastoid foramen' inasmuch as it represents the orifice of exit of the seventh cranial nerve ('portio dura') from the petrosal bone, here in the cavity of the middle ear, there being none such upon the outside of the skull. Thus, in the dry skull of a bird, the hard parts of the tympanic cavity, including the eustachian tube, can readily be inspected from the outside; even the limits of the proötic and opisthotic bones can be determined by the site of the fenestra ovalis, and the ossicula auditus be seen *in situ*. To see these things in the human or any ordinary mammalian ear, requires special preparations, as they lie in a tympanum which is itself at the bottom of a contracted tube. Details of mere size and shape aside, the above general description of the passageways will apply pretty well to any bird, and should suffice for recognition of the parts; though the number and variety of the irregular pneumatic openings (comparable to those of the human mastoid cells) may be puzzling at first sight.

(*To be continued.*)

ON THE KINETIC THEORY OF THE SPECIFIC HEAT OF SOLIDS.

In a paper entitled 'Kinetic considerations as to the nature of the atomic motions which probably originate radiations,'[1] the writer has given reasons in support of the hypothesis that different chemical atoms are all composed of the same kind of ultimate atoms, which are in every respect equal and similar. Reasons were also given, tending to show that the vibrations of these ultimate atoms originate luminiferous and thermal radiations. And further, supposing radiations to originate in the vibrations of equal and similar ultimate atoms which are set in vibration by the collision of moving molecules, an attempt was made to prove that two unlike masses of gas which are in thermal equilibrium by radiation will also be so when mixed; i.e., when the equilibrium depends upon the collisions of the molecules rather than upon radiation.

The object of the present paper is to consider the probable physical state of solid bodies, especially as to the amount of energy distributed among the different degrees of freedom possible in such bodies, and to show that the same hypothesis of equal ultimate atoms would cause solids which are in thermal equilibrium by radiation to be also in thermal equilibrium when brought into contact, i.e., when the equilibrium depends upon the collisions of the molecules.

Let us notice, in the first place, what is apparently the mechanical significance of Dulong and Petit's law, which may be stated thus: the amount of heat which must be imparted to a chemical atom of a simple solid body to increase its temperature one degree is approximately the same for all the elements. Neumann has further shown, that, for compound solids, those of similar chemical composition require approximately the same amount of heat per chemical atom, but the amount is less than for simple solids. There are, however, a very few unexplained exceptions to these laws, which are due possibly to uncertainty as to atomic weights.

The mechanical explanation of these experimental laws seems to be contained in the statement, that, in simple solids, cohesion and chemism are one and indistinguishable; or, to express it otherwise, we may say that the molecules of simple solids are monatomic, the cohesion being, of course, much greater in some solids than in others.

That this is a correct conception of the relations of the atoms of a simple solid, is made probable by various facts, among which this may be mentioned, — mercury and cadmium, which are known to be monatomic as gases, as solids fulfil Dulong and Petit's law, and are therefore in the same physico-chemical state

[1] SCIENCE, ii. 76.

as other simple solids. Another fact is that already mentioned, viz., the specific heat of compound solids per atom is less than that of simple solids; and to this it may be added, that the specific heat of simple solids is less when the volume is made smaller by hammering, compression, or cooling, which facts will be considered more at length later.

It is shown in the kinetic theory of gases, that, when molecules of unlike gases are mixed, the mean progressive energy of each molecule is the same, whatever its weight.

Now, when a gas is in contact with a solid, will the collisions of the gaseous molecules with those of the solid cause the latter to have the same mean progressive energy of vibration as those of the gas? That will depend largely upon the duration of the collision. If the time occupied by a collision is so brief that only a small portion of a vibration of the solid molecule is described during the collision, then the laws of impulsive forces may be applied, according to which the effect of the finite forces acting during the interval may be neglected.

In case the collision is brief, the distribution of the mean kinetic energy between the molecules of the gas and solid will be very nearly the same as between different gases, and the mean kinetic energy of a simple solid molecule will differ little from that of a gas at the same temperature.

In cases, however, in which the modulus of elasticity of the solids considered is so great as to make the period of vibration of the molecules also brief, their mean kinetic energy would be materially smaller than in the previous case; and, if a solid could be found whose molecules were immovably fixed, no vibratory energy whatever could be imparted to its molecules.

Now, Dulong and Petit's law seems to show that all simple solids, even those having the highest modulus of elasticity, have an elasticity so small, compared with that brought into action between molecules at the instant of free collision, that the distribution of kinetic energy is approximately the same as if the body were gaseous and monatomic. But since the laws of perfect elasticity require that the mean potential energy shall be equal to the kinetic, it follows that the specific heat of a simple solid should be approximately twice that of a monatomic gas at the same temperature and of the same atomic weight.

The actual specific heats of mercury and cadmium gas would be of great interest in this connection, were they known, even though they could only be determined at temperatures far removed from those of their solids.

The foregoing statement has been based upon the assumption that any degree of freedom which suffers partial constraint, as do the degrees of freedom of translation of a gaseous molecule when it becomes solid, will have for that reason less kinetic energy imparted to it during molecular collision. This subject has been treated somewhat at length in previous papers upon the kinetic theory; but in this connection it may be useful to make a quotation from Thomson and Tait: "If a set of material points are struck independently by impulses, each given in amount, more kinetic energy is generated if the points are perfectly free to move, each independently of all the others, than if they are connected in any way."[1]

This mechanical theorem not only has special application to the partial constraints introduced into the freedom of motion of molecules when they change from a gaseous to a solid state, but it applies, also, to the additional constraints introduced into the degrees of freedom of solid atoms when those atoms become more closely bound together by chemism into groups, i.e., into molecules. Evidently, the bonds of union between the atoms of a compound solid molecule are such that these degrees of freedom are considerably more constrained than those which unite the atoms of different molecules; so that, in compound solids, the forces of cohesion and chemism are different, and quite distinguishable the one from the other.

Now, what, according to the mechanical theorem above quoted, is the effect of introducing the additional constraints required in order to group a simple solid, or mixture of simple solids, into molecules, and thus make it a compound solid? The effect will be to diminish the mean kinetic energy of the system as derived from the impacts of the molecules of any gas surrounding it. This is, in fact, what occurs, as appears from the experimental truth previously mentioned, — that the specific heat per atom of compound solids is less than that for simple solids. How much the specific heat per atom is diminished should depend upon the intensity of the chemical attraction, which certainly must be much greater than the cohesion between atoms of simple solids, to cause such marked deviations of specific heat per atom as compound solids exhibit. This result, when combined with that arrived at in connection with the discussion of Berthelot's law, in my paper upon 'An extension of the theorem of the virial,' etc., to the effect that the heat evolved in chemical decomposition is greater

[1] *Nat. phil.*, art. 315.

the greater the attractive force, enables us to enunciate the following law, the truth of which I am at present unable to verify for want of sufficient experimental data: those solids, other things being equal, which evolve the greater amounts of heat of chemical decomposition in changing from simple mixtures to compound solids, are those which have less specific heat per atom. The phrase, 'other things being equal,' in the above statement, refers to the fact that similar compounds which are chemically similar are in strictness comparable. Many other circumstances, moreover, besides want of chemical similarity, may, in special cases, mask the experimental results; yet the truth of the law should be clearly recognizable in any general comparison of specific heats with the heat of formation of compound solids.

Similar principles evidently apply to the cases in which simple solids are permanently decreased in volume by hammering or compression; for then greater cohesive forces are brought into action, and the specific heat is diminished. It remains to be shown, in conclusion, that thermal equilibrium, which has been established by collisions of gaseous and solid molecules, will continue to exist when its continuance depends upon radiations between equal and similar ultimate atoms which are set in vibration by molecular collisions; or, to state it differently, it remains to be shown that the ultimate atoms of a gas and a solid in contact, each have the same mean vibratory energy with respect to each of their degrees of freedom with respect to each other. This appears to be a direct consequence of the laws of constrained motion which have been considered in this and previous papers. It is only necessary that the impacts of a pair of solid molecules with each other should be such as to mutually impart and receive the same mean amounts of energy as would those of a gaseous and a solid molecule at the same temperature, to cause it to be a matter of indifference whether a given solid molecule is struck by another solid molecule or by a gaseous molecule; and, when so struck, each ultimate atom will receive its proper proportion of energy, whether it form part of a solid or of a gaseous molecule.

It is my intention to return to this subject hereafter, and to treat the vibrations of ultimate atoms more at length, in the hope of being able to show, more precisely than has been done so far, how the characteristic differences in the spectra of solids and gases arise.

H. T. EDDY, Ph.D.

CLIMATE IN THE CURE OF CONSUMPTION.—I.

THE prevalence of phthisis pulmonalis is such a well-attested fact, that to adduce statistics to prove it would seem to be labor thrown away. Since the eradication of small-pox in consequence of the introduction of vaccination, phthisis heads the list as the prime cause of the large mortality. The insurance companies recognize the fact, and the statistics of the New-York mutual life-insurance company show, that between the ages of twenty and thirty years the mortality from phthisis is thirty-three per cent of the whole mortality. The U. S. census for 1870 shows that in the state of Maine the mortality from consumption was fifty per cent for the same ages.

Equally well known is the belief in climate as a cure for the disease. There are certain well-recognized climatic conditions known to be favorable to the prophylaxis and cure of the disease. This knowledge is largely empirical, based upon trial and observation; but there is, underlying it, a substratum of conviction, that is justified, on the one hand, by careful clinical observations, and, on the other, by facts ascertained by carefully conducted experiments.

The writer proposes, in the thoughts to be presented, to make these various elements his tests in searching out a desirable climate in the United States for the cure of phthisis. He offers, as his data for forming an opinion, carefully compiled tables, furnished by the Signal-service bureau, U.S.A.; and he wishes to emphasize the fact, at the outset of his remarks, that a climate may become desirable quite as much by comparison as on account of its intrinsic properties; that even though it may not possess in itself all desirable qualities, yet it may contain so many as to be, by comparison with others, the climate par excellence. With this thought in view, the writer has prepared tables embracing all the chief resorts in this country for phthisical invalids, — tables embracing a range of the whole country, from Jacksonville to St. Paul, and from Boston to Los Angeles.

He has given the data for Augusta, Ga., as the best substitute for Aiken, S.C., at which place there is no signal-station; and in doing so he thinks that he is presenting data which will fairly represent the climatic conditions of Aiken.

He wishes to gratefully acknowledge his indebtedness to the chief signal-officer, U.S.A., to the observers at each of the stations included in the tables, and especially to Sergeant F. M. Neal of the Denver station, for their kindness in furnishing him with the data from which the tables are compiled.

TABLE I.

STATION. The figures giving the elevation represent the height of the barometer above sea-level.	I. Elevation.	II. Mean 10 yrs. Barometer.	III. Mean 4 yrs. Relative humidity.	IV. Mean 4 yrs. Absolute humidity.	V. Mean 10 yrs. Precipitation.	VI. Mean 10 yrs. Temperature.	VII. Mean 5 yrs. Prevailing wind.
Augusta, Ga.	183	30.140	69.2	4.56	48.98	64°.2	N.E.
Jacksonville, Fla.	43	30.030	69.0	5.38	55.94	69°.2	N.E.
Boston, Mass.	142	29.840	68.5	2.66	49.47	48°.5	W.
Newport, R.I.	34	29.950	74.3	3.07	50.20	50°.3	S.W.
New York, N.Y.	164	29.857	70.2	3.02	42.70	51°.3	N.W.
Philadelphia, Penn.	52	30.084	68.8	3.17	41.89	53°.2	S.W.
Chicago, Ill.	661	29.317	69.2	2.77	35.47	49°.3	S.W.
St. Paul, Minn.	811	29.133	67.3	2.23	29.59	43°.9	S.E.
Denver, Col.	5,294	24.778	45.8	1.81	14.77	49°.1	S.
Santa Fé, N. Mex.	7,046	23.263	41.4	1.61	14.17	48°.5	E.
Salt Lake, Utah	4,348	25.644	40.3	1.76	17.52	51°.8	N.W.
Los Angeles, Cal.	350	29.647	65.8	3.77	18.97	59°.8	W.

Elevation.

The effect of a rise in elevation is to diminish the atmospheric pressure. The method of measuring this effect is by means of the mercurial barometer. Disregarding the variations attributable to changes in temperature, humidity, and latitude, it can be broadly stated that the barometer will fall one inch for a rise of 857 feet above sea-level, two inches for a rise of 1,743 feet, three inches for a rise of 2,661 feet, etc.; or, for the purposes of a rough calculation, it may be said that the barometric depression is one inch for every thousand feet of elevation. This depression would indicate a diminution in atmospheric pressure of one-sixth in weight for an elevation of 5,000 feet; or, to state this fact in another way, the atmospheric pressure at sea-level being 15 lbs. to the square inch, at 5,000 feet it would be one-sixth less, or 12½ lbs. To illustrate: if the pressure on the entire surface of the body of a man of middle size be 35,560 lbs. at sea-level, at 5,000 feet it would be 29,635 lbs., a diminution of nearly three tons.

The question for us to consider is, what effect this diminution of pressure has upon the vital functions with reference to the cure of phthisis.

1. *Effect on circulation.* — The heart is a muscular organ, habituated to expend a certain amount of force, which may be roughly estimated as 75,000 kilogrammetres, or 542,475 foot-pounds, *per diem*. To accomplish this work at sea-level, the heart makes 72 beats a minute, or 103,680 beats a day. Allowing, now, an increase of two beats a minute for every thousand feet of elevation, at 5,000 feet there will be an increase of 10 beats a minute, or 14,400 beats a day, — an increase in work equal to about 74,744 foot-pounds in a day.

This in itself would prove that such an elevation is to be avoided in those cases where an enfeebled heart is struggling to overcome the disadvantages produced by organic lesions.

What effect does this increase of heart-work have upon the circulation? The rapidity of circulation is influenced by the force and rapidity of the heart's beat, and by the diminution of the peripheral resistance. At an elevation of 5,000 feet, each of these causes would be at work. To just what extent they work, in producing given results, it is impossible to say; but, allowing that the peripheral resistance and the force of the heart's beats remain the same at 5,000 feet as at sea-level, an increase in frequency of ten beats per minute would indicate, that, on account of this one factor, the blood would make 29,622 additional circulations through the system *per diem*. What effect would this have upon the disease in question?

It is a frequent remark that both waste and repair are more rapid at high altitudes than at sea-level. Experience amongst physicians shows that cases of fibrinous pneumonia are more acute and more rapid in their results at

high altitudes than at sea-level. My own experience is, that resolution in such cases is more rapid, and that the chest 'clears up' sooner. May not this be explained on the ground of the increased rapidity of circulation? We know that the clearing-up is brought about by the expectoration of the morbid products of the exudation, and further, and chiefly, by their absorption into the circulation. If this be true of an acute trouble, is it not also applicable to a chronic asthenic one?

Further, the increase of rapidity in the circulation means that the same blood is brought more frequently to the lungs to be oxygenated, — an increase in the number of times, which we have seen to be equal to 29,622 additional times, *per diem*. This would indicate an increase in the activity of the metamorphosis of tissue, and therefore an increased vital force. This is clinically perceptible in the exhilaration that invalids experience on coming to higher altitudes, and by the increase in appetite dependent upon the demand for material to meet the additional metamorphosis.

There is the other side, however, which must be alluded to. An increase in the rapidity of the circulation means an increased flow or tendency of blood to the diseased parts, involving, as it does, a greater activity of these parts. This is temporarily noticeable in every case of phthisis pulmonalis coming to higher altitudes, and is evidenced by an increased expectoration. This, as we have said, may be beneficial by assisting to remove the morbid products; but in enfeebled cases, where the ravages of the disease are great, it may be highly injurious in assisting the already great breaking-down of the tissues. Again: there is an increased demand for oxygen dependent on all of these causes, and, in cases where the amount of lung-tissue involved is so great as to cause a considerable embarrassment of the respiration, this additional strain is not desirable.

2. *Effect on respiration.* — We must now study another effect produced upon the system by an increase in elevation, and that is, the effect produced upon the respiration. Here, too, we shall have to speak of gross results, and leave minutiae unexplained.

Experience shows that the respirations are deepened and fuller, and that they are, at first, at least, increased in number.

This can be explained somewhat in the following way. The nervous action, the effect upon the respiratory centre, etc., is so complicated, that, despite its importance, we shall leave it without attempting its solution; and we shall only attempt an explanation of the quasi physical or mechanical results.

The lungs are elastic bags suspended in a closed cavity. During inspirations the respiratory muscles draw the ribs upwards, enlarge the cavity, and produce a partial vacuum, in consequence of which the air rushes in to fill up this vacuum, and the lungs are inflated.

It is evident that in inspiration the respiratory muscles, in raising the chest-walls, displace a certain amount of air, and overcome a certain resistance due to atmospheric pressure, and that these muscles, accustomed to exert a given amount of force to overcome this resistance (which may be roughly measured by the difference between the positive pressure on the outside and the negative pressure on the inside of the chest-walls), would continue to exert this force, even though the resistance were diminished. As a result of this, we should expect either a greater expansion of the chest from the same expenditure of force, or an increase in the number of inspirations.

The beneficial results of a greater depth of inspiration will be more clearly seen if we contrast it with the bad results of diminished respiration. Ruehle, in Ziemssen's Cyclopaedia, says, —

"The diminished respiration in the upper parts of the lungs, and the exaggerated respiration in the lower parts resulting from this cause, serve to explain the very general fact that pulmonary consumption almost always begins at the apices of the lungs. But there is probably another cause in the peculiar position of these parts. They project from three to four centimetres above the clavicles; and this projecting portion, being situated outside the chest, is subjected to the pressure of the external air. The supraclavicular region sinks in during deep inspiration, and consequently the inspiratory expansion of the apices is less than that of other parts of the lungs."

It is evident, if Ruehle be right, that a diminution of atmospheric pressure means a greater expansion of the apices, due to a diminution of atmospheric pressure bearing on those parts, and also a greater expansion of the entire chest.

There is, however, a theory often advanced, that the greater depth of respiration is due to the fact, that, in consequence of the diminution of the amount of oxygen dependent upon the decreased atmospheric pressure, an individual will have to breathe more frequently and deeper to gain the amount of oxygen necessary for aerating the blood. The theory, it seems to us, is misunderstood, and the question needs investigation.

We have shown that there is an increased demand for oxygen dependent upon an increased tendency of the blood to the lungs.

It is known, also, that the transpiration of gases through tubes, which the bronchi really are, is hindered by a diminution of pressure, and that in consequence of this, in a given time, under the same conditions of expansion, etc., less air will enter the lungs at 5,000 feet elevation than at sea-level. This is another cause why the respirations should be either deeper or more frequent.

It is further known that the osmose of gases through a thin septum, as in the lungs, is less rapid the less the pressure, or, in other words, that the rapidity of the osmose of gases is dependent upon the pressure to which they are subjected. This being so, as the density of the oxygen and carbonic acid in the blood is nearly constant, if the density of these gases in the air be diminished, there will be an effect produced upon the rapidity of their osmose. In the case of oxygen, it is claimed that an individual will get less of it at 5,000 feet elevation than the system requires, and that, unless the conditions of respiration be changed, there will be a 'starvation of oxygen,' i.e., an asphyxia. In consequence of all this, it has been claimed that a greater depth and frequency of respiration is demanded to meet this want.

In regard to the osmose of oxygen, we know, that, even though there be a hindrance due to the diminished density of the gas in the air, there is still, on the other hand, an increased rapidity of the circulation, which would favor osmose; and it may be assumed that the effects of these two conditions counterbalance one another.

In regard to a starvation of oxygen being produced by a diminution in the amount of the gas at a higher elevation, the *rationale* is somewhat as follows: there exists in the atmosphere, under all pressures, 23 parts by weight of oxygen. At sea-level, there are 130.4 grains of the gas in every cubic foot, while, at 5,000 feet, this amount will be diminished one-sixth, so that there will only be 108.6 grains to the cubic foot. The question then is, whether a density of 108.6 grains to the cubic foot of air will produce a starvation of the gas in the human economy.

It has been estimated that the tension of the oxygen of the venous blood of the dog is 2.9%, or 22 mm., of mercury. It has been further estimated that the tension of the oxygen of the pulmonary air-cells is at least 10% of the atmospheric pressure, which, at 5,000 feet, would amount to 63.3 mm. of mercury; so that it is evident, that, even under this diminution of pressure, the difference between the density of the oxygen in the inspired air and in that of the venous blood brought to the lungs is sufficiently great to admit of a free osmose.

Further than this, we know that the amount of tidal air passing in and out of the chest of an average man is 500 cc., or 31 cubic inches. Allowing 17 respirations to the minute, this will make 510 litres, or 18 cubic feet, of oxygen inspired *per horam*. At 5,000 feet this air would contain 1,955 grains of oxygen. Now, the absolute absorption of oxygen at sea-level is only five per cent of that contained in the air, and the amount that is absolutely needed each hour, at sea-level, is only 117 grains. As the absolute demand for oxygen is only 117 grains each hour, and as the actual amount contained in the inspired air at an altitude of 5,000 feet is, for the same time, 1,955 grains, it is evident that here, again, the supply is greatly in excess of the demand, and that the term 'starvation of oxygen,' as explanatory of the increased depth and rapidity of the inspirations at high altitudes, is a misnomer.

But in addition to the absorption of oxygen there is the elimination of carbonic acid to be accounted for. It is evident, that, as the tension of the gas in the venous blood coming to the lungs is nearly constant, any diminution of its tension in the air will favor its osmose from the blood to the air, and that the effect produced upon the osmose of this gas by rise of elevation is the reverse of the effect upon the osmose of oxygen.

In concluding this part of our subject, we wish to emphasize the fact that we think that the benefit to be derived from simple elevation, in cases of phthisis pulmonalis, is to be attributed largely to the greater depth of the inspirations, and consequently to the greater distension and activity of all parts of the lungs (the diseased apices as well as the healthy bases), and to the increased elimination of morbid products brought about by the increased rapidity of the circulation.

3. *Ozone.* — In addition to the foregoing reasons for favoring a high altitude in the cure of phthisis, we wish to consider, further, the influence of elevation upon the ozone of the atmosphere.

The assertion is generally made, that, as "we ascend heights, the amount of the ozone rapidly increases;" and yet there does not seem to have been any direct experimentation on this point. If there be more free ozone, it may be due, not to any increased production over that of lower levels, but rather to a diminished consumption. Further than this, the

starch and iodide test is so dependent upon other elements than the simple presence of ozone, that it is not thoroughly reliable. It is also open to the error of reacting to substances other than ozone. Still, admitting the statement that there is more nascent ozone at high elevations, the explanation of its action in the cure of phthisis is still to be sought. Some rather visionary theorists, as it seems to the writer, claim that it finds a direct admission to the diseased spots in the lungs, and, by its poorer oxidizing, it burns up *in loco* the morbid products.

We should rather attribute its influence to the fact, that, where ozone exists free, there is no decomposing matter to be oxidized. It seems to us to be indicative of the existence of pure air, rather than a direct agent in destroying the morbid products in the lungs.

4. *Immunity from phthisis.* — Another argument in favor of elevation in the cure of phthisis is, that at certain heights there exists an immunity from the disease. The disease is not endemic at such elevations.

This is in the nature of negative evidence; but it is certainly valuable as an element of prophylaxis, and we think that it can be applied as an argument in favor of cure. Ruehle (*op. cit.*) says, "A height of *at least* 1,800 or 2,000 feet seems to be requisite for this purpose. Phthisis is rare on the Hartz, Styrian (in Purzgau), and Swiss mountains." Jaccoud (Flint's Practice of medicine, p. 296) "states that the observations for fifteen consecutive years warrants him in asserting, that, in Alpine situations elevated 4,000 feet, tuberculosis is unknown; and especially is this true of villages at an elevation of 5,500 feet." Dr. Irwin reports for Fort Defiance (6,500 feet), north-western New Mexico, "During a service of some seven years in New Mexico and Arizona, I never saw or heard of a case of tuberculous disease amongst the native inhabitants of those territories." And Dr. Denison, in his work entitled 'Rocky Mountain health resorts,' writes, "After having quite thoroughly canvassed the subject among physicians of Colorado, I place the altitude of approximate immunity of this state at 6,000 feet."

Taking a mean of all these quotations, we may safely assert, that, broadly speaking, an altitude of from 5,000 to 6,000 feet affords an approximate immunity from this disease.

5. *An aseptic atmosphere.* — Lastly, we will speak of the influence of elevation in the cure of phthisis in producing an aseptic atmosphere. In these days of germ-theories and of Koch's experiments, we cannot but give emphasis to this element of antisepsis as an element of prophylaxis and cure of phthisis. Professor Tyndall's experiments show the abundance of germs floating in the air at sea-level, and an entire absence of such germs at the altitude of the 'Belle Alp' hotel (7,000 feet). Whether a lower elevation will furnish this aseptic atmosphere has not been proven experimentally; but it would seem to be reasonable to argue that an elevation corresponding to that of immunity from phthisis would furnish such an atmosphere.

Résumé. — There are other elements, such as humidity of the air, temperature, precipitation, etc., more or less dependent upon elevation, which we shall have occasion to speak of more at length. But, to make a *résumé* of our study to this point, we can say that a rise in elevation increases the heart-beat and the rapidity of the circulation, thereby hastening the absorption of the morbid products in phthisis, and increasing the metamorphosis of tissue, and hence the vital force; that it likewise produces greater depth of respirations, and a more healthy action of the diseased portions of the lungs; that it gives a purer air, and affords an approximate immunity from the disease; and, finally, that it affords an aseptic atmosphere, in which the Bacillus tuberculosis does not exist. The extent of elevation desirable for the production of this effect can be stated to be at least 5,000 feet.

Having arrived at these conclusions, it remains for us to apply them to our subject. By consulting table I., columns i. and ii., it will be seen, that, of all the resorts for the cure of phthisis in this country, the eastern slopes of the Rocky Mountains alone furnished the desirable elevation. The distance between Denver and Santa Fé is in the neighborhood of 375 miles in extent. Throughout this whole extent, pleasant locations for invalids are to be found at elevations varying from 5,000 to 6,000 feet.

(*To be continued.*)

HISTOLOGY OF INSECTS.

INSPIRED by Weissmann's well-known researches on the post-embryonic development of insects, Viallanes has studied the structure and changes of various tissues, principally in Musca vomitoria, but also in other insects during their metamorphoses. His results occupy nearly an entire volume,[1] and make an important addition to knowledge, the more welcome because the author deals chiefly with those tissues which have heretofore been least worked

[1] Vol. xiv. sér. vi. of Ann. sc. nat., zool.

upon. The long memoir embodies a large number of valuable data, the outcome of work which we believe to be thorough and careful. The collation of the literature is good, but not complete, some omissions being important. We are unable to give here more than the chief general conclusions.

The skin of the larvae studied consists of a single layer of large flattened cells, covered externally by the hard chitinous cuticula (containing lime in Stratiomys), which is smooth in Musca and Eristalis, but divided in Stratiomys into fields corresponding to the cells. Below the cells, and lying directly against them, is a thin anhistic membrane, which is comparable to the basal membrane in crustacea and adult insects.

The peripheral nervous system is of great interest. Between the integuments and the muscles of the larvae are found peripheral ganglia, which do not belong either to the ventral chain or to the stomatogastric system. No analogous observation has hitherto been made upon insects. The peripheral ganglia of the larva of Tipula are very remarkable from their regular disposition and their symmetry: there is a pair in each segment. Those of Musca are irregularly scattered between the skin and the muscles. Analogous ganglia are found in Eristalis, but are localized in the plexus, whence spring the nerves of the special sense-organs in the anterior region of the body. In the description of the peripheral nerves the author adds little to what was previously known.

The sensory nerves end in two ways, — either by a connection with sensory hairs of the epidermis, or with free terminations. In the former case the axis-cylinder dilates, at the base of the sensory hair, into a bi-polar ganglion cell. The sensory hair is a conical hollow process of the cuticula. It is secreted by a special, large, slightly modified epidermal cell, the protoplasm of which fills the cavity of the hair, and lines its base. The distal prolongation of the bi-polar cells unites with the protoplasm of the hair-cell, and does not run directly to the hair. This apparatus appears to subserve touch, smell, etc. The free terminations are found beneath the epidermis, as thread-like prolongations of a very rich dermal plexus, formed by very numerous multi-polar anastomosing nervous cells. (Besides the description of similar structures in other animals cited by Viallanes, *cf.* Canini and Gaule, SCIENCE, ii. **279**.)

Involuntary striated muscles. The larval heart is histologically comparable to a vertebrate capillary, being formed of flat cells soldered border to border. In the protoplasm of these cells, muscular fibres are formed, so that the cells are at once comparable to the endothelium and muscularis of the capillary. Within each single cell the fibrilla begins and ends with a thin disk or stria; therefore the space between the two disks is the unit of the fibril. In young larvae the heart is a simple tube without lateral openings. The striated muscles of the digestive tube are probably histologically identical with those of the heart, i.e., modified single cells; but Viallanes was unable to make out the cell-limits. In the walls of the stomach of Tipula is an intramuscular ganglionated nerve-plexus, which probably innervates the muscles; but the final terminations were not seen. This is regarded as confirmatory of Ranvier's law (*Leç. d'anat. génér.*, 1880, 463).

In regard to the voluntary muscles the following conclusions are drawn: the fibrillae of insects are homologous with those of vertebrates, although the latter are indivisible, while in insects certain fibrillae (of the wing-muscles) may be decomposed into fibriculae. In insects, as in vertebrates, the fibrillae are united into 'colonettes,' or little clusters, being closely cemented together by a homogeneous and continuous substance, into which neither protoplasm nor nuclei ever penetrate. In vertebrates a large number of colonettes are united within a common envelope, the sarcolemma, to form the fibre or primitive bundle. In insect larvae this disposition is maintained, but in the wing-muscles the sarcolemma is absent; the primitive bundle then consists of a few colonettes (Musca), or even of one colonette only (*cf.* Ciaccio, SCIENCE, i. **247**, whose paper is not cited). In the leg-muscles there is but a single colonette in each fibre, and the sarcolemma is scarcely developed. As regards the motor plates the following points are noted : 1°. In the larva of Stratiomys chamaeleon, each of the fibres, constructed on the vertebrate type, has several Doyère's cones, to the summit of each of which runs an axis-cylinder accompanied by a nucleated sheath. Before innervating the muscle, the nerves form a plexus; in the cone the axis-cylinder forms a terminal arborization by successive dichotomous branchings inside the sarcolemma; the fundamental substance contains neither granular matter nor nuclei. 2°. In Tipula there is a similar arrangement, but only one cone to each fibre; the terminal arborization is much more extended, and bears nuclei; and the basal substance of the cone is granular, and nucleated as in the terminal plates of Amniota. 3°. In the caudal muscles of Eristalis and the leg-muscles of Dytiscus, each fibre of which contains only a single colonette, the motor nerves form no arborization, but break up into their constituent fibrils as soon as they reach the sarcolemma.

The second part of the memoir deals with the very remarkable changes in the larval tissues at pupation. The corpuscles of the blood of the larva are embryonic cells analogous to the leucocytes of vertebrates, and are found in the same form in the pupae. The muscular fibres of the larva disappear at the commencement of pupal life, and in two ways : — First, by 'évolution régressive :' the nuclei of the muscle become spherical, and each surrounded by a coat of protoplasm, thus becoming a muscle-corpuscle, which proliferates, and gives rise to a great number of rose-colored granules, which multiply until the muscular substance entirely disappears, as if it supplied nutriment to the granules; these last finally separate, and spread themselves through the body cavity. Second, by degeneration: the nuclei keep becoming rarer until they all disappear, and meanwhile the contractile substance disappears as if

dissolved away on the outside. In consequence of these processes, the body cavity is charged with a quantity of matter resembling the vitelline elements of birds. The cells of the so-called fat-body produce, during the first days of pupal life, numerous granules, which enlarge, and are ultimately set free by the rupture of the cell-membrane. These granules arise independently of the nucleus, but closely resemble small cells. The cells of the tracheae and salivary glands do not disappear at the time of metamorphosis, as has been thought, but, on the contrary, they proliferate by endogenous cell-formation, the parent cell being first enlarged; the parent nucleus is finally discharged; the embryonic cells thus generated separate, and fall into the general body cavity. The *körnchenkugel* produced by pupal histolysis, and described by Weissmann, are of two kinds, and do not arise from the disintegrated matter, as supposed; but the smaller are derived from the muscle-corpuscles, the larger from cells of the fat-body. The epidermis of the larval head and thorax dries up and falls off. It is not immediately replaced by the definite cell-layer, but first by a thin cuticle, which Viallanes considers to be probably the thickened basement membrane of the larva.

Part third treats of the histogenesis of the tissues of the imago. The skin of the head and thorax is developed from Weissmann's imaginal disks. In the description of these, Viallanes follows Ganin in general, but he thinks that the mesoderm of the disks is formed at the expense of some of the embryonic cells in the body cavity. Other points are also brought forward, among which we note especially that the wing of the pupa contains at first numerous tracheae, which disappear before the end of the stage. In the abdomen, also, there are imaginal disks, four in each segment, and formed by local thickenings of the epidermis; all other parts of the epidermis or hypoderm degenerate, and are resorbed. The disks form two layers, the outer making the new epidermis, and the inner the mesoderm; the disks grow at their borders until they everywhere meet, and form a continuous tissue. The method of regeneration is the same as in the thorax, except that the disks are developed later: the difference assumed by Weissmann and Ganin is not real. The author compares the imaginal disks with the plates in Pilidium.

The internal muscular mass of the thorax is derived from a single anlage, composed of little cells embedded in a small amount of homogeneous basal substance. This anlage then separates into six cords, corresponding to the definite muscles; these grow by peripheral accretion; the muscular substance is then differentiated around the cells, which are disposed with great regularity in the midst of the colonettes, becoming, in fact, the muscle-corpuscles (the necessity of omitting a fuller account is much regretted. — *Rep.*). The muscles of the legs are derived from the mesoderm of the imaginal disks; the general process of their histogenesis, despite many interesting differences, is the same as that of the wing-muscles. The author makes an excellent comparison between the unicellular muscles (heart, stomach) and the pleuricellular (wings, legs), or, as we might name them, the mesenchymal and myothelial muscles.

Nearly a fifth of the entire memoir is devoted to the development of the eye. The brief *résumé* (p. 302–305) is the most succinct and perfect account of the structure of the compound eye with which we are acquainted. In the first section the structure of the developed eye of the pupa, before it becomes pigmented, is described. The following is the author's table of the parts of the visual apparatus: —

Œil composé . . .	Cornée à facettes. Couche des cellules cristalliniennes. Couche des rétinules ou rétine. Limitante postérieure de l'œil composé.
Couche des fibres post-rétiniennes.	
Lame ganglionnaire	Limitante antérieure de la lame ganglionnaire. Couche des cellules ganglionnaires. Couche des fibres en palissade. Limitante moyenne de la lame ganglionnaire. Couche des fibres nuclées. Limitante postérieure de la lame ganglionnaire.
Couche des fibres préganglionnaires.	
Ganglion optique .	Névrilemme. Couche des cellules en chapelets. Croissant du noyau central. Éventail du noyau central. Écorce grise du ganglion optique.

Concerning the development of the eye, we give the following conclusions. In the larva, before metamorphosis, the eye is represented by three parts, — the imaginal disk of the eye proper, the neural stem, and the optic ganglion. The disk of the eye comprises the same three layers as the other imaginal disks. Before the metamorphosis of the larva, the superficial cells of the exodermic layer become enlarged and elongated, and acquire a strong affinity for coloring-matters; they are the optogenic cells. This change begins in the centre, and spreads towards the periphery of the disk. The mesoderm of the disk of the eye, unlike the other two layers, is different from the corresponding portion of other disks, since it is composed of fine nerve-fibrillae mingled with nuclei; by teasing, it can be shown that each fibril is connected with the inner end of an exoderm cell. The nervous stem unites the disk of the eye with the optic ganglion, and is composed of the nerve-fibrils mingled with nuclei. The optic ganglion is constituted by the outer portion of the brain; its nucleus consists of white, its cortex of gray, matter; in the lateral portion of the cortex, is the complex anlage of the *lame ganglionnaire*, in which all the principal constituent parts of the definite *lame ganglionnaire* can be recognized. At the moment of metamorphosis the following phenomena occur: the provisory layer of the disk of the eye disappears, the exoderm enlarges, its borders unite with the neighboring disks, its cuticle becomes the faceted cornea, and its optogenic cells each form, by the known process, an elementary eye. The anlage of the *lame ganglionnaire* emigrates from the optic ganglion, then enlarges, and spreads out so as to intervene between the ganglion and the eye. The

details of the differentiation of the *lame* are carefully described.

I cannot conclude this notice without referring to the admirable manner in which this valuable memoir is written, and the great clearness with which the facts and conclusions are presented.

CHARLES S. MINOT.

EXPERIMENTS TO DETERMINE THE GERMICIDE VALUE OF CERTAIN THERAPEUTIC AGENTS.

IN the *American journal of medical sciences* for April, Dr. Sternberg gives an account of his study of this important question. The objects of the author were, —

To ascertain the exact value, as germicides, of some of the agents most frequently employed in medical and surgical practice, with a view to the destruction of pathogenic micro-organisms, hypothetical or demonstrated.

To compare this value, established by laboratory experiments, with the results of clinical experience, for the purpose of ascertaining what support, if any, the germ-theory of disease receives from modern therapeutics.

Assuming that the active agent in infective material is a living micro-organism, or 'germ,' disinfection will be accomplished by those chemical agents only, which have the power of destroying the vitality of this organism. We require to know: —

a. What is the absolute germicide power of various disinfecting agents, in order to select the best with a view to economy and efficiency;

b. Are all disease-germs destroyed by these agents in the same proportion? and, if not,

c. What agents are the most available for special kinds of infective material?

In therapeutics we should know, in addition to this: —

d. What is the minimum quantity of each of these agents which will restrict the multiplication of each specific disease-germ in a suitable culture-medium? — this with reference to medication, with a view to accomplishing a like result within the body of an infected individual.

Evidently, any thing like a complete answer to these questions is quite impossible in the present state of knowledge, and we must content ourselves with such partial or approximate answers as can be obtained by laboratory experiments upon the comparatively small number of pathogenic organisms which abound in organic liquids undergoing putrefaction.

The experiments were conducted by using small sealed flasks containing bouillon free from micro-organisms. The smallest quantity of a fluid containing such organisms introduced into one of the flasks would cause it to 'break down' within twenty-four hours, it being exposed during this time to a temperature of 100° F.

To test the germicide power of a chemical reagent, living bacteria are subjected to its action in a known proportion for a given time, and are subsequently used to inoculate sterilized bouillon in one of the flasks. Failure to multiply in this fluid, when exposed for twenty-four hours or more to a temperature of 100° F., is evidence that reproductive power — vitality — has been destroyed by the reagent used. On the other hand, failure to disinfect, i.e., to destroy the vitality of the bacterial organisms used as a test, is shown by the 'breaking-down' of the culture-fluid.

Standard solutions of the reagents to be tested are prepared with distillled water. The germs are exposed, in small glass tubes, to the action of these agents for two hours. The tubes are sterilized in the flame of an alcohol-lamp immediately before each experiment; they are open, and covered by a bell-glass during the time of exposure.

At the end of the time of exposure, a small quantity of the fluid from one of the tubes is introduced into a flask containing sterilized bouillon, and this is exposed to a temperature of 100° F. for twenty-four hours.

The micro-organisms which have been used in the experiments herein reported, to test the germicide power of the reagents named, were obtained from the following sources: —

a. A micrococcus from gonorrhoeal pus.

b. A micrococcus from pus obtained from an acute abscess (whitlow) at the moment that it was opened by a deep incision. This micrococcus is morphologically identical with the preceding.

c. A pathogenic micrococcus, having distinct morphological characters obtained from the blood of a septicaemic rabbit.

d. Bacterium termo, and other bacterial organisms (micrococci and bacilli) from 'broken-down' beef-tea which had been freely exposed to the air.

In the following table, which is arranged according to the germicide value of the agents named, all experiments are given in which the micrococcus from pus was used as a test.

Mercuric bichloride (0.005 per cent), efficient in the proportion of one part in	20,000
Potassium permanganate (0.12 per cent), efficient in the proportion of one part in	833
Iodine (0.2 per cent), efficient in the proportion of one part in	500
Creosote (0.5 per cent), efficient in the proportion of one part in	200
Sulphuric acid (0.5 per cent), efficient in the proportion of one part in	200
Carbolic acid (1 per cent), efficient in the proportion of one part in	100
Hydrochloric acid (1 per cent), efficient in the proportion of one part in	100
Zinc chloride (4 per cent), efficient in the proportion of one part in	50
Tinc. ferri chloridi (4 per cent), efficient in the proportion of one part in	25
Salicylic acid dissolved by sodium borate (4 per cent), efficient in the proportion of one part in	25
Caustic potash (10 per cent), efficient in the proportion of one part in	10
Citric acid (12 per cent), efficient in the proportion of one part in	8
Chloral hydrate (20 per cent), efficient in the proportion of one part in	5

The following-named reagents, as far as the experi-

ments go, are not shown to have any germicide value; viz., —

	Per cent.
Fowler's solution failed in the proportion of	40
Sodium hyposulphite failed in the proportion of	32
Sodium sulphite, exsiccata, failed in the proportion of	10
Ferric sulphate (saturated solution) failed in the proportion of	16
Potassium iodide failed in the proportion of	8
Liq. zinci chloridi failed in the proportion of	8
Boracic acid (saturated solution) failed in the proportion of	4
Zinc sulphate failed in the proportion of	20
Sodium borate (saturated solution) failed in the proportion of	4
Sodium salicylate failed in the proportion of	4

Having ascertained the germicide value of certain reagents for a single micro-organism, the question arises as to whether we are justified in assuming that other organisms of the same class, and especially pathogenic bacteria, will be destroyed by the same reagents in like proportion, or, in other words, whether we can generalize from the data obtained. It is evident, that, if each of the reagents named gives identical results with several distinct species of bacteria, we shall be justified in assuming that the value obtained will be constant for other organisms, known or unknown, of the same class; whereas, if marked differences are found as to the vital resistance of different bacterial organisms to these reagents, no generalization will be possible, and the value for each distinct organism of the class can only be fixed by experiment. To solve this question, experiments have been made as follows: —

a. Upon the micrococcus of pus.

b. Upon the micrococcus of septicaemia in the rabbit.

c. Upon bacterium termo, in its active motile stage, as found in a fresh culture.

d. Upon the bacteria in broken-down beef-tea which had been freely exposed to the air, and in which all active development had ceased.

The results show, that, in general, those reagents which destroyed the vitality of the micrococcus from pus are destructive to organisms of the same class, and that their relative value as germicides is not changed when a different micro-organism is used as the test of this value. Moreover, the reagents which were found to be practically valueless as germicides in the first series of experiments — e.g., ferric sulphate, sodium sulphite, and hyposulphite, boracic acid, etc. — proved to be equally without value when the test was extended to other micro-organisms of the same class. But the reagents found to possess decided germicide power have, in some cases, a different value for different organisms: in other words, the vital resistance of different bacterial organisms to the reagents in question is not in all cases the same.

Thus, sulphuric acid failed to destroy B. termo and the micrococcus from pus in the proportion of 0.25 %; but one-fourth of this amount (0.06 %) destroyed the vitality of the septic micrococcus.

Caustic potash destroyed the septic micrococcus in the proportion of 2 %, but failed to kill the micrococcus of pus in four times this amount (8 %). The value, as a germicide, of the solution of ferric sulphate and sulphuric acid in water, which has been extensively recommended by sanitarians as a disinfectant, evidently depends upon the sulphuric acid which the solution contains. To insure the destruction of all bacterial organisms and of the reproductive spores of those species which multiply by spores as well as by transverse fission, such a solution should be used in sufficient quantity to subject the material to be disinfected to the action of the acid in the proportion of at least five per cent for a period of two hours.

The quantity of carbolic acid used to accomplish the same result should not be less than five per cent, for it is necessary to keep on the safe side; and we do not know, at present, whether all of the pathogenic bacteria, hypothetical or demonstrated, form spores or otherwise. In the case of the anthrax bacillus and of Koch's bacillus of tuberculosis, this has been proved to be true; and we have ample experimental evidence to show that these reproductive bodies possess very great resistance to heat and to those chemical reagents which destroy bacterial organisms in their ordinary condition of rapid growth and multiplication by fission.

Evidently, therapeutic value — assuming the correctness of the germ-theory — cannot be gauged by germicide power alone, for it is possible that a reagent which possesses this power in but slight degree, or not at all, may nevertheless be capable of restricting the development of pathogenic organisms, and thus limiting their power for mischief.

The following table shows the percentage required to destroy vitality, and also that required to prevent the development of the micrococcus of pus: —

Reagent.	Percentage required to destroy vitality.	Percentage capable of preventing development.
Mercuric bichloride	0.005	0.003
Iodine	0.2	0.025
Sulphuric acid	0.25	0.12
Carbolic acid	0.8	0.2
Salicylic acid and sodium biborate	2	0.5
Alcohol	40	10
Ferric sulphate	Failed in saturated solution.	0.5
Boracic acid		0.5
Sodium biborate		0.5

An inspection of the table shows that the potent germicides in our list restrict multiplication in quantities considerably less than are required to destroy vitality. In the case of iodine the difference is eightfold; in that of carbolic acid, fourfold; in that of sulphuric acid, twofold, etc.

We also see that the agents at the bottom of the list, — ferric sulphate, boracic acid, and sodium biborate, — in the proportion of five-tenths per cent, prevent the multiplication of bacterial organisms, and are consequently antiseptic agents of value, although in saturated solution they fail to kill these organisms.

In the case of ferric sulphate, and also of zinc sulphate and zinc chloride, this power to prevent the development of micro-organisms seems to be due to

precipitation of the organic material in the nutritive medium rather than to any direct action upon the living organisms, which, as we have seen, are not killed by a far greater quantity of the reagent.

The conclusions at which Dr. Sternberg arrives, are, that the vital resistance of bacterial organisms to chemical reagents differs, within certain limits, for different species. And certain species show special susceptibility to the germicide action of particular reagents; e.g., the septic micrococcus to alcohol, and B. termo to boracic acid.

There is, therefore, reason for supposing that different pathogenic organisms may differ in like manner, as to susceptibility to the action of various reagents administered medicinally with a view to their destruction. Nevertheless, the comparative germicide value of the reagents tested is the same for the several test-organisms, and, allowing certain limits for specific peculiarities, it is safe to generalize from the experimental data obtained in the practical use of these reagents as disinfectants. But it must be remembered that the resisting power of reproductive spores is far greater than that of bacterial organisms in active growth (multiplication by fission), and the data obtained for the latter cannot be extended to include the former.

The antiseptic value of the reagents tested depends upon their power to prevent the multiplication of putrefactive bacteria; and this is not necessarily connected with germicide potency, for some reagents which fail to kill these micro-organisms are, nevertheless, valuable antiseptics, e.g., ferric sulphate and boracic acid.

Clinical experience has demonstrated the value of all the potent germicide reagents tested in one or more of the diseases which there is the most reason to believe are due to the presence of pathogenic micro-organisms in the *primae viae*, in the blood, or in the tissues; e.g., intermittent-fever, typhoid-fever, dysentery, erysipelas, syphilis, etc. The 'germ-theory' as to the causation of these diseases receives, therefore, very strong support from modern therapeutics; but the experiments do not justify the belief that any one of the reagents tested can be administered as a specific in germ-diseases generally. This also accords with the results of clinical experience, and makes it possible to believe that the specific, self-limited diseases are also 'germ' diseases.

LETTERS TO THE EDITOR.

The practical value of soil-analysis.

IN *Bulletin* lvi. of the New York agricultural experiment-station, Dr. Sturtevant gives the reasons for which the station declines to make soil-analyses 'for the purposes of the individual farmer;' summarizing them in the statement that such ayalyses "can offer no solution of the problem of what fertilizer, and how much, to apply."

Were this statement made in a somewhat less general and absolute manner, I should have no fault to find with it; for in the case of the long-cultivated fields of the state of New York, which have been subject to indefinitely varied culture-conditions and the use of fertilizers, the cases in which chemical analysis alone would point with any degree of certainty to the true cause of failure to produce profitable crops would be exceptional; and the station would be likely to be overrun with requests for an indefinite amount of comparatively useless routine work.

But when Dr. Sturtevant broadly adds his denial "that analyses of soils can give us definite information concerning their productiveness," he seems to go beyond the limits justified by the record, and beyond what the context following would appear to show he intended to say. If the clause above quoted were to read, instead, "while denying that analyses of *cultivated* soils can give us definite information regarding their *present* productiveness," I should agree with him so far as the great majority of cases is concerned, — so much so, that it is only exceptionally that I undertake the analysis of a cultivated soil, but usually go back to its virgin ancestor for information as to its general character; and from this, and the usually simple history of its cultivation, pretty definite inferences as to the prominent wants even of a cultivated soil can in very many cases be deduced, as is proved by the practical results. Dr. Sturtevant's own statement as to the frequency and consequent practical importance of such inquiries would seem to justify the taking of some pains to approach its solution, before proclaiming an absolute *non possumus*.

As for virgin soils, which over wide areas have been subject to uniform or uniformly variable conditions, it is *a priori* reasonably presumable, and I think experience confirms the inference, that, other things being equal, the amount of available plant-food, and therefore the durability of a given soil under the usual culture, without replacement, is sensibly proportional to the plant-food percentages shown by the usual method of analysis. Whether or not other things are really equal can only be ascertained by intelligent examination in the field as well as in the laboratory; and soil-specimens taken by non-experts rarely fulfil this condition.

While, therefore, believing that Dr. Sturtevant's action in this matter is well advised under the circumstances, I nevertheless believe that my contrary practice in regions but sparsely or recently settled is at least equally well justified, and that the importance of affording the settler at least an approximate insight into the present and ultimate durability of his soil, and its general character and adaptations, is so great as to justify a considerable public expenditure, upon a well-considered plan carefully carried out by competent persons both in the field and in the laboratory, even with our present limited knowledge of the chemistry of soils — which, I cannot but remark, is not likely to be increased very rapidly if the composition of soils serving for culture-experiments continues to be ignored, as has so largely been the case heretofore. The prime importance of the presence of a certain minimum percentage of lime, for example, is manifestly so great, that no experimenter can afford to be ignorant of the presence or absence of a proper supply of that substance in his soil; and the cases in which analysis shows the extreme scarcity or extreme abundance of lime, phosphates, or potash, in virgin soils and subsoils, are far more frequent than the contemners of soil-analysis suppose. In the former case the practical value of the indication is too obvious to be overlooked, and is amply attested by the results following the application, e.g., of phosphate fertilizers in such cases. We might not be able to detect the addition thus made to the phosphates of the soil by the most careful analysis; but the fact that the soil is naturally poor in phosphates will remain a fruitful truth forever after.

I trust that the record which will be shown in the census report of cotton production, now in press, will form a convincing illustration of the legitimate uses of soil-analysis. E. W. Hilgard.

University of California, Sept. 1, 1883.

Do humming-birds fly backwards?

The Duke of Argyll, in his Reign of law (p. 145), lays it down in italics, that '*no bird can ever fly backwards*.' He mentions the humming-bird as appearing to do so, but maintains, that, in reality, the bird falls, rather than flies, when, for instance, he comes out of a tubular flower. But this morning, while watching the motions of a humming-bird (Trochilus colubris), it occurred to me to test this *dictum* of the duke; and, unless my eyes were altogether at fault, the bird did actually fly backwards. He was probing one after another the blossoms of a Petunia-bed, and more than once, when the flower happened to be low down, he plainly rose, rather than fell, as he backed out of and away from it. I stood within a yard or two of him, and do not believe that I was deceived.

It may not be amiss to add that the Duke of Argyll's objections seem to be purely theoretical, since the 'Reign of law' was published in 1866, and it was not till 1879 that the author came to America and saw his first living humming-bird.

Bradford Torrey.

Boston, Sept. 14, 1883.

Wright's ice-dam at Cincinnati.

I notice on p. 320 of Science, vol. ii. no. 31, an inaccurate report of what I said at the Minneapolis meeting, which does injustice both to Mr. Wright and to myself, and which I would beg to have corrected.

The reporter makes me speak slightingly of Mr. Wright's discovery of the ice-dam at Cincinnati, as not sufficing to explain our Pennsylvania terraces. On the contrary, I expressed my admiration for the discovery as furnishing precisely the explanation we need for the *local-drift* terraces of the Monongahela, and the *rolled-northern-drift* terraces of the lower Alleghany, Beaver, and upper Ohio rivers.

The reporter probably mixed this up with what I said afterwards respecting the *rolled-drift* terraces of eastern Pennsylvania, which only reach a height of 800′ A. T., in Northumberland county, and require some explanation, perhaps, quite unconnected with that which Mr. Wright certainly furnishes in a most satisfactory manner for the 800′ to 1,100′ A. T. terraces of the Ohio River basin. J. P. Lesley.

Second geological survey of Pennsylvania,
Philadelphia, Sept. 15, 1883.

Erratic pebbles in the Licking valley.

While engaged in tracing the outcrop of 'Clinton ore' in eastern Kentucky, in the fall of 1882, I became interested in the pebbles, which in certain localities, and up to a certain height, were very abundant in the surface-soil.

Most abundant were rounded quartz pebbles, probably from the millstone grit. Somewhat less abundant were fragments of chert, showing little or no wear derived from the sub-carboniferous limestone. Still less abundant, though by no means rare, were some from the carboniferous, often containing characteristic fossils. They were confined, so far as I could determine, to the valley of the Licking and its larger tributaries. Vertically, they range from the river-bottoms to the top of the table, formed by the upper Silurian rocks, which borders on the Devonian escarpment; so that these tables are quite uniformly covered with the material.

The distribution of the material is such as could only have been made while the valley was temporarily occupied by a lake. I was therefore led, though with some hesitation, to suppose that the glacier must have crossed the Ohio at Cincinnati, damming the river. I was not at the time aware of the labors of Mr. Wright in tracing the glacier across the Ohio.

Having now the certainty that there was a dam at the required point, I think I may have no hesitation in saying, that, during a portion of the glacial period, the valley of the Licking was occupied by a lake which overflowed laterally, and whose bottom became littered with materials brought from the mountains of eastern Kentucky by floating ice. They are most abundant where the ice may be supposed to have had freest access.

Terraces which might have been expected are wanting in the region in which my observations were made. Possibly they may be found in other parts of the valley, especially above; their absence in the region in question being due to the fact that only small portions of the region would have reached above the lake-level, which, by their disintegration, could furnish the material for terraces.

The overflow was probably to southward, but I could not search for it. Could it be traced, the amount of erosion might give some data for an estimate of time. G. H. Squier.

Trempealeau, Wis., Sept. 14, 1883.

Depth of ice during the glacial age.

In the issue of Science for Sept. 7, reporting my paper at Minneapolis, I am made to say, that, during the glacial period, the ice was indeed "600 feet over New England, and very likely of equal depth over the area to the west." I said 6,000 feet over New England. The evidences of glaciation are distinct upon the Green Mountains to a height of nearly 5,000 feet. The lower summits of the White Mountains, like Carrigain (which is 4,300 feet above the sea), are covered with transported bowlders; and there can be little question that some found by Professor Charles Hitchcock, within a few hundred feet of the summit of Mount Washington, were transported thither by glacial agency. Such is the evidence for New England.

For the region north of Pennsylvania and the Ohio River, direct evidence of such a great depth of ice is naturally wanting; but, according to Ramsay, glacial scratches are numerous upon the summit of Catskill Mountains in New York, at an elevation of 2,850 feet above the sea. In southern Ohio there are numerous places where the ice, within a mile or two of its farthest extension, surmounted elevations which are about 500 feet higher than the plains to the north of them. I see no reason why it should not have been as deep over the bed of Lake Erie as over the region to the north of the White Mountains, though there are there no glaciometers like Mount Washington to measure the height of the frozen mass.

G. Frederick Wright.

Oberlin, O., Sept. 13, 1883.

The 'stony girdle' of the earth.

In your issue of Sept. 7, just received, you are kind enough to insert a synopsis of the two abstracts of papers which I sent to the Minneapolis meeting. Allow me the space necessary to make a correction and some brief explanations. We are required to furnish these 'abstracts' to suit a printed form of small note size, which is apt to lead to small chirography: hence I suppose the mistake in reading and printing the title.

It should read, 'stony girdle,' and was in inverted commas to show that the name did not originate with me. My special object was to call attention to its being, in a great measure, the same belt which forms the prime-vertical when the pole of the land-centre at Mount Rosa is brought to the zenith. The unfavorable comments to which you allude have force as a general rule ; namely, that closet geology is not comparable to observations in the field. Yet *all generalizations* may be called closet geology, as being the result of a large number of facts collected in the field, and compared subsequently. As it would, however, be presumptuous in any one to offer generalizations who had not had somewhat extended opportunities for observation, I may be permitted to mention, as some justification, those I have enjoyed. In North America my observations, partly in special work, partly during travel, have ranged from Rainy Lake, north of Lake Superior, to Saltillo, in Mexico, and from the Atlantic states to the head waters of the Gila, in Arizona. In the eastern continent, I travelled from the north of Scotland to Cairo in Egypt, ascending Etna, and spending the vacations of three summers, during college-life, in Switzerland among its mountains, ranging subsequently from western France to the Crimea. In 1824 I saw the 'Perte du Rhône,' where that river disappeared for miles, and then re-appeared,—a phenomenon no longer to be seen, as the superincumbent rocks, some years later, caved in, and converted the subterranean into a subaerial bed for that fine stream.

In 1829 I visited the scene of the catastrophe at New Madrid; and while granting a local subsidence for the immediate cause, as claimed in the able paper by Dr. Macfarlane, of which you give an abstract, I am compelled to believe that the remote cause was due to a seismic movement, felt, as Mallet states, at least two hundred miles from New Madrid, and, indeed, affecting large and more distant areas about that time, as mentioned in Key to geology, p. 77.

These opportunities, in connection with the specimens and notes of reference brought home, permit a review of general geology, which I thought might enable me to present to the student of geography and geology some broad principles and truths into which the details subsequently obtained by him might be appropriately fitted: hence the paper read at the Boston meeting, showing that the eastern trend of each continent was distant one-fifth of the circumference of the globe from its adjoining continental trend; also that each continent presented a central focus, from which a circle with radius of 36° would embrace the land proper, — sometimes excluding a peninsula, such as Hindostan, sometimes including adjacent islands, as those of Madeira, Canary, and Cape Verd, as belonging to the main continent, Africa. The Montreal papers were designed to show the important seismic fissurings radiating from the pole of the land-centre; also the relation between solar and terrestrial dynamics, where seismic phenomena are transmitted along great circles coinciding with the sun's apparent path, or along belts of the earth's crust which are secondaries to the ecliptic.

The occurrences of the last few weeks seem to corroborate the generalization offered, inasmuch as Ischia is on the 30° fissure from Rosa, at no great distance; while Java and the Straits of Sunda, as well as Guayaquil, more recently disturbed, are on or close to the prime-vertical.

If these generalizations belong rather in the category of instruction for the student than of contributions to science, perhaps my twenty-five years of natural-science teaching may present some excuse. Certainly, my great aim and desire are to arrive at important scientific truths, especially general laws in the dynamics of our globe. RICHARD OWEN.

Mr. Morse's papers at Minneapolis.

A number of errors have been made in the report of my papers which were read at the Minneapolis meeting.

In the paper on an apparatus for warming and ventilating apartments, the statement that the temperature of a hall was raised 40° above the outside temperature is incorrect. I said that the air, as it entered the room *from the heater*, had been raised 40° above the outside air.

In the paper on the methods of arrow-release, I spoke of the English method, which was probably that of the Saxon, and said that American archers followed the English. The Japanese never use thumb-rings, to my knowledge. The Koreans, Chinese, Manchu Tartars, and Persians use the thumb-ring.

A more serious mistake occurs in the report of my paper on the indoor games of the Japanese. I said very distinctly, that, in the game of chess, pieces captured could be used by the capturer against his opponent. In comparing the Japanese games with ours, I made no allusion to seven-up or whist. With every one I regard whist as next to chess in character as a highly intellectual game.

You will confer a great favor by publishing these corrections. EDW. S. MORSE.

Salem, Mass., Sept. 16, 1883.

Evidences of glacial man.

In SCIENCE, no. 32, p. 384, the statement is made, respecting Miss Babbitt's Minnesota finds, that "thus far, at best, the glacial workman is known only by his chips." What better evidence, I would inquire, is needed, if those chips are of artificial origin?

Is not this sufficient? Are not shavings and sawdust as good evidence of men working in wood, today, as are the planes and saws they use? From the very nature of the case, it is unreasonable to find as abundant and easily recognized evidence of man in drift-deposits as upon the surface-soils; yet this is what some of those present at the Minneapolis meeting of the American association for the advancement of science seemed to require.

In the case of the 'paleolithic' implements of the Delaware River valley, other evidence than the chipped stones has been found. The human tooth, lately described in detail in the Proceedings of the Boston society of natural history, is, of itself, evidence of man's presence at the time the gravels, in which it occurred, were laid down. Other human remains have also been found.

A word, too, with reference to the implements. These are nearly all as unmistakably artificial as the most finished arrow-head. Objects of identical character are found among the relics of the recent Indians, and are not questioned. Why, then, should a similar class of objects, found in gravel-deposits that antedate the superincumbent surface-soils, be questioned?

There is no doubt overshadowing the existence of man in the Delaware valley as long ago as the close of the glacial period: his presence, then, is not merely 'a theory advanced by Dr. Abbott,' as you suggest, but a fact susceptible of actual demonstration.

Professor Mason, in his address (in the same issue), asks, "What is the real import of such discoveries as those of Dr. Abbott and Professor Whitney in establishing the great antiquity and early rudeness of

the American savage?" Speaking for myself, I would suggest that his question contains its answer. My discoveries have established the glacial age of man on the Atlantic seaboard of America, and at that time his culture was that stage known as 'paleolithic.'

CHAS. C. ABBOTT, M.D.

Trenton, N.J., Sept. 18, 1883.

THE ALPHABET.

The alphabet, an account of the origin and development of letters. By ISAAC TAYLOR, M.A., LL.D. 2 vols. London, *Kegan Paul, Trench, & Co.*, 1883. 16+358; 398 p. 8°.

MR. TAYLOR has produced an admirable work on the interesting subject of alphabetic writing. It abounds in wealth of collected material, down to the very latest discoveries (some of them of the utmost importance). By lavish and well-chosen illustration it puts this material before the apprehension of the reader or student with the most desirable clearness; and its digest and criticism of former opinions is made with impartiality and independence of judgment, while the author adds abundantly of new views, and arguments to support them. No other existing work of a like character can bear any comparison with it; and it deserves to have, as it doubtless will attain, a wide circulation and popularity.

In the main, these volumes are filled with the history of our own alphabet and its relatives, or of the ancient Phoenician with its descendants and probable ancestor, since other systems of alphabetic writing are comparatively insignificant in number and in importance. The Chinese characters are not alphabetic, although one or two derivatives from them (as the Japanese *kata-kana*) have that character. The cuneiform mode of writing ended its career in an alphabetic system, the Persian; but all the peoples using cuneiform passed over, more than two thousand years ago, to the side of the Phoenician. There have been other hieroglyphic schemes, in the old world and the new, that made advances, no one can say just how far, toward alphabetism; but they are long since perished without descendants. All these, together with such theoretic basis as he chooses to lay for the science, Mr. Taylor despatches in the first chapter (seventy pages) of his first volume; the rest is devoted to our alphabet: the various kindred Semitic forms of it being treated in the former volume, and the Indo-European forms, with the few outside stragglers, in the latter, under the divisions of Greek, derivatives of Greek (Italian, Coptic, Slavonic, Albanian, Runic, Ogham), Iranian, and Indian. The method is not to be condemned, although we might have desired a more ample theoretical introduction. The fundamental principle of alphabetic history is distinct, and briefly statable: all writing begins necessarily with the depiction of scenes and objects, or is purely pictorial; it everywhere tends to pass over into a depiction of the names of objects; and, when it has fully reached that condition, it has become alphabetic. There can be no such thing as an alphabet not starting from a pictorial stage, any more than a spoken language without an initial imitative root-stage. But while in language we can only get back by inference to such a state of things, because the beginnings of language are so remote from us, in writing we find the pictorial stage abundantly represented.

Whether that stage is discoverable in the actual history of our own alphabet, is a question not yet absolutely settled. Every step by which our familiar letters go back to the primitive Semitic alphabet, usually called by us Phoenician, is traced out with the utmost distinctness. The Phoenician is purely, though defectively, alphabetic. It must, then, have come from a pictorial original. Three such systems of writing are found in its neighborhood, — Egyptian, cuneiform (the perhaps sufficient, though rather scanty, evidences of whose hieroglyphic origin are given by our author), and the recently discovered and still obscure Hittite. Did it come demonstrably from one of these, or has it an ancestor now lost to us? As is well known, De Rougé's work, published less than ten years ago, attempted to show its derivation from Egyptian, from hieratic characters, of known hieroglyphic originals; and his view is widely, though by no means universally, accepted. Mr. Taylor is a firm believer in it, and sets it forth with much clearness and force. We find ourselves unable fully to share his conviction. De Rougé endeavored to prove more than was reasonable, and found it so easy to prove all he undertook, that his very success casts a shade of unreality over the whole comparison. We may allow that his identifications are both possible, and, as a whole, plausible quite beyond any others yet made. Yet whereas the derivation of the Greek or of the Arabic alphabet, for example, is past all doubt, and he would rightly be passed by as a time-waster who should attempt to re-open the question, no reproach can attach to the scholar who, unconvinced by De Rougé, should try to find another and better solution of the problem, as some are actually doing. Mr. Taylor overstates the desirableness of acquiescing in the

best solution hitherto discovered; the right to doubt an inference not yet made certain is a precious and indefeasible one. It would be highly gratifying to regard the derivation of Phoenician from Egyptian as not less certain than that of English from Phoenician, since then we should have followed up the history to its very beginning; for the character of the Egyptian as a wholly original mode of writing, carrying on its face the evidence of its steps of development from the initial stage, is beyond dispute. Considering that Mr. Taylor holds the hieroglyphics to be the antecedent phase of Phoenician letters, we wish that he had made his exposition of the system somewhat fuller, and especially that he had told in more detail how he regards the alphabetic value of certain of the hieroglyphs as having been arrived at: the point is by no means so clear as were to be wished.

It would take far too much space to go through the book and notice all the points of special interest in it; but attention may be called to a few. Mr. Taylor has a new and well-supported theory as to the Mediterranean alphabet from which the Germanic runes were taken: he holds it to have been the Greek of the Euxine colonies and Thrace, transmitted in peaceful intercourse along the commercial route of the Dnieper, some centuries before the Christian era. His discussion of the Ogham cryptograms is less satisfactory. The Glagolitic (an early Slavonic) alphabet receives from him a suggested explanation which has met with general favor. The earliest Semitic monuments — the sarcophagus of Sidon, the Moabite stele, the recently discovered Siloam inscription — are fully treated, the last being given in facsimile. Some of the most original parts of the author's work lie in the discussion of the South Semitic alphabets and their derivatives. It is to them that he traces the immense group of the alphabets of India by a theory which wears a more plausible and acceptable aspect than any other yet suggested; it must, of course, stand the test of time, and of examination by other experts, before it can be admitted as final. Even in so old and well-worked departments as the varieties of Semitic and Greek writing and their mutual relations, Mr. Taylor brings to light much that is new and interesting, laying under contribution the most recent finds, and combining them with independence of judgment and sound sense. There is nowhere any effort at brilliancy or show of profundity: sober, earnest work is the keynote of the treatise, which in this respect compares favorably with certain other recent publications, French and German, on the same subject.

In conclusion, we may notice adversely a point or two. The now accepted explanation of Pehlevi, as needing to be read out of its Semitic signs into Iranian words, should not be credited to 'the sagacity of Professor Haug' (ii. 239). That explanation was distinctly offered by the veteran Westergaard, in the preface to his Zendavesta, in 1854, when Haug was fresh from the university; and in the latter's earliest article 'on the Pehlevi language and the Bundepesh,' published in the same year, there is to be found no hint of the doctrine.

It is hardly correct to ascribe the success of right methods in paleography in any measure to Darwinism (ii. 363). That every successive phase of a historical institution is the outgrowth of a preceding phase, and differs little from it, is a truth long coming to clear recognition and fruitful application in every department of historic research, prior to and in complete independence of any doctrine of evolution in the natural world. Only error and confusion have come of the attempts made to connect Darwinism and philologic science. On the other hand, Mr. Taylor appears to make a too mechanical application of the doctrine of historical development in denying altogether the possibility of an element of free invention in alphabetic growth. Man is capable of devising something a little different from, or like and additional to, what he has already won and knows how to use. One who has a language can invent another, regarded by him as an improvement on the former: the thing has happened repeatedly, and is no violation of the law of gradual and unconscious growth of human speech. So, notwithstanding the law of alphabetic development, a man who practises various modes of writing can devise a new one, for cryptographic or tachygraphic purposes, or other. And a community that is receiving and adapting an alphabetic system from another community may, in like manner, well enough add a sign or two of its own device: hence the question whether our X is an out-and-out invention of the Greeks, or a differentiated K, is one of paleographic probabilities, not to be settled in favor of the latter alternative by denying the possibility of the former; and so in other like cases.

The number of interesting questions to which this work furnishes a trustworthy reply is surprising; and, while sparing of notes, it yet gives references sufficient to set upon the right track any one desirous of investigating more fully the matters with which it deals.

THE FOSSIL FLORA OF GREENLAND.

Die fossile flora der polarländer. Von Dr. OSWALD HEER. Vol. vii. Zürich, *Wurster*, 1883. 275 p., 62 pl. 4°.

THIS volume contains, 1°, the flora of the upper cretaceous schists of Patoot; 2°, the tertiary flora of Greenland; 3°, a short memoir on insects' remains found in connection with the plants (cf. SCIENCE, i. 1095); 4°, general remarks on the affinities of the plants in relation to their geological age and the climatic circumstances indicated by their characters; 5°, a memoir by Steenstrup on the geology of the localities where remains of plants and coal-deposits have been found; 6°, the marine fauna, with descriptions of the species of invertebrate animals found especially in connection with the plants of Patoot.

This last locality represents the upper member of the cretaceous of Greenland; the lowest being that of Kome, the middle that of Atane. The flora of Patoot has a predominance of conifers and ferns, no Cycadeae, and few monocotyledons, about one-half of the plants being dicotyledons. The table of distribution, which represents the whole cretaceous flora of Greenland, enumerates 335 species, — 88 for Kome, 177 for Atane, and 118 for Patoot. From the characters of the plants, the schists of Kome are referable to the Neocomian. Atane, whose flora is related to that of the Dakota group of Kansas, represents the Cenomanian, while Patoot is apparently Senonian. Most of its species are related to those of Atane, only a few being identified with eocene species from Sezanne and with some miocene types. The plants of the tertiaries of Greenland have been procured from twenty different localities. Their description is also followed by a table of distribution. Of the 282 species enumerated, 33 are known from the tertiary of North America, 10 of them from the Laramie group. The greater number are identified with species found in the lower miocene of Europe, the Aquitanian group, whose flora is widely represented in most of the states, from Hungary to England and France, and from Italy to North Germany. This tertiary flora of Greenland has been predominant, and has preserved its characters for many thousands of years; for the lower strata, where its remains have been found, are separated from the upper, which have the same kinds of plants, by thousands of feet of basaltic masses the deposits of which have been continuous for long periods of time.

In the general remarks considering the climatic conditions which have governed the vegetation as indicated by the characters of the flora, Heer says, that in 1868, from data derived from the determination of 105 species of plants, he had estimated the mean temperature at 9° C.; but now the tertiary flora of Greenland, known by a larger number of plants of various types, — among them a palm, species of Laurus, Magnolia, Diospyros, Sapindus, Zizyphus, etc., whose analogues are now found in Virginia, the Carolinas, etc., — indicates by its constituents a mean temperature of 10° to 11°.

The few mollusks and star-fishes, mostly found at Patoot, have been determined by the French paleontologist, de Loriol. He considers them to be related to some of those described by Meek from the Fox Hill group. Steenstrup's memoir on the geology of the localities where the plants have been found is precise and detailed. It is illustrated by a number of good sections.

The work is accompanied by a map of the western coast of Greenland between 69° 15′ and 72° 30′ north latitude.

THE CHESAPEAKE OYSTER-BEDS.

Report on the oyster-beds of the James River, Virginia (etc.). Coast-survey report for 1881. Appendix, no. 11. By FRANCIS WINSLOW, U.S.N. Washington, *Government*, 1882. 87 p., 22 pl., 3 maps. 4°.

AMONG the various investigations of the U.S. coast survey since its organization, the bearing of which is not confined to their geodetic, topographic, or hydrographic relations, the present publication is conspicuous.

By direction of the late superintendent Patterson in 1878, an investigation of the oyster-reefs or natural beds of the Chesapeake and vicinity was entered upon by Lieut. Winslow with the coast-survey schooner Palinurus. The intention was to determine the limits of the beds, their hydrographic features, the nature of the natural and artificial changes which they undergo, and the present distribution of living oysters upon them. It was proposed to thoroughly investigate a limited area, subsequent extension of the work to all the Chesapeake beds to be left for future decision. Under the term 'Chesapeake' we include here not only the beds in the waters of the bay specifically so called, but those in the extensions of salt water from the bay into the various inlets, arms, rivers, etc., adjacent to and continuous with it.

Originally the oyster beds or 'rocks,' as

they are not inappropriately termed by the fishermen, were patches of suitable ground upon which these bivalves had lived for ages, and, dying, left their shells to be overgrown by successive generations. Matted together by this living cement, the successive layers of dead shells and associated *débris* gradually rose toward the surface, covered with distorted, misshapen bivalves in masses like those of the Floridian 'coon oysters.' These beds were separated pretty sharply from the adjacent muddy bottoms, a differentiation which the vertical increase tended to intensify. Horizontal increase doubtless took place, but very slowly. From an economical stand-point the oysters upon these beds were inferior on account of their inconvenient shape and excessive crowding. Among the various conflicting statements drawn out by investigations into the oyster-industry, one fact seems to be generally admitted by fishermen and by experts; namely, that a moderate amount of dredging over the original 'oyster-rocks' was beneficial. This dredging extended the area of the beds, 1°, by dragging the dead shells and 'cultch' over upon adjacent muddy bottoms, and placing it where new spat could settle and grow; and, 2°, by distributing the living oysters more sparsely over the ground, so that they had a chance to grow into regular and even shape and relatively larger size. It is recognized by dealers, — even when the dredging has been carried on, as at present is the case in the Chesapeake, to a disastrous extent, — that the few remaining oysters which are obtained are of larger size and finer flavor than common.

Since the trade in oysters began, the beds have undergone great changes in area and productiveness, until, at present, in two years, on certain beds, the product has diminished in the ratio of six to one, the market-price has nearly doubled, while the demand is constantly increasing. If it were not for supplies received from other sources, the oyster-eaters of cities about the Chesapeake would have to pay nearly European prices for their favorite shell-fish.

It is true that there are numerous laws on the statute-books of Maryland and Virginia; that police steam-launches and men have been enlisted and a sort of war enacted, in time of peace, by state authorities, — all ostensibly in protection of the oyster-beds. Actually the laws are a dead letter; dredging is boldly carried on in close time before the eyes of the 'oyster police,' without the offenders being molested; and the only occasion for active measures arises when a Virginia dredger trespasses in Maryland waters, or *vice versa*. Gore is then apparently in demand, but, in spite of vehement protestations, turns out almost as scarce as oysters.

It was upon this state of things that Lieut. Winslow entered, when he undertook this work without previous experience, or any knowledge of the biological questions involved, except such as might be gleaned from the valuable little work of Moebius on the North Sea fisheries of Europe. Many of the observations which he was directed to take, are, in the present state of our knowledge, productive of no definite result, though eventually they may prove very useful. Thus, observations of the specific gravity and temperature of the water at the bottom and surface, when the total depth was only a few feet, may be said to be almost absolutely fruitless. It is well known that our common oyster flourishes in water which varies at different seasons from the freezing-point to 80° F., and that similar differences of specific gravity must occur between the extremes of its geographical range. Consequently the differences, in summer, of fractions of degrees of temperature in the water over oyster-beds, are of no consequence whatever. What these changes of temperature may signify, when taken in connection with the act of spawning or the development of the embryo, is quite another question, purely biological, and which can be properly treated only by a biological expert of high rank and long experience.

The result of these superfluous observations and detailed description of each individual bed, even condensed as they are, as far as possible, by the author, is to overload the text with details of no interest, and thus to obscure to the reader the value of the investigation, the really interesting facts, and the merits of the investigator, which are neither few nor small. They will amply repay any one who has patience to wade through the mass of details, and pick out those of present value, of which there are many. Space forbids any attempt to summarize them. A large area of the beds was delineated, and the approximate number of marketable oysters upon them determined. Profiles of the beds were obtained in numerous instances, and the character of the subsoil, or bottom under the beds, determined as were the conditions of sedimentation. Nearly all the beds examined are described in detail. Valuable biological data were obtained through the efforts of Dr. W. K. Brooks and Mr. H. J. Rice, most of which have been already made

public in other ways. Much information on the general topic was obtained by questioning the fishermen, whose replies, though biassed by self-interest, may be set off against one another, and a residuum of useful facts obtained.

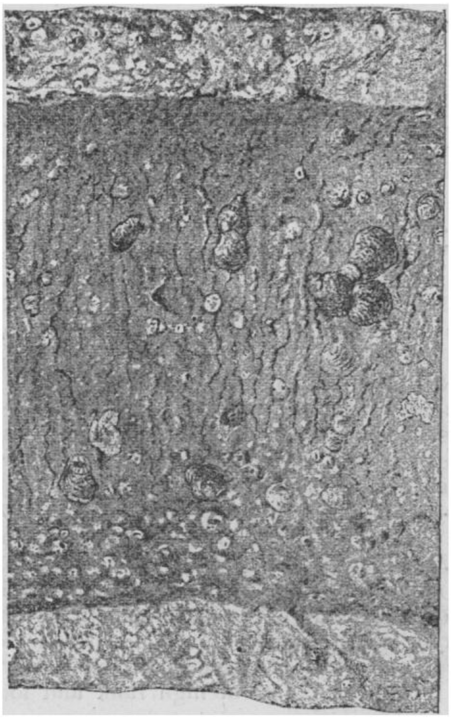


FIG. 1.—LOWER SIDE OF TILE EXPOSED JULY 9–AUG. 2. (TWO-THIRDS NATURAL SIZE.)

To our judgment, apart from the survey and delimitation of many oyster-beds, the most important results of this investigation are, 1°, the determination of the approximate quantity of oysters to the square yard over a great portion of the beds; and, 2°, the data in regard to the rapidity of growth of the young mollusks as indicated by the tile-collectors, and the proportion of mortality among them from causes not yet fully explained. The determination of the small mollusk Astyris, as an enemy of the infant oyster, though not conclusive, is of interest, and, if finally confirmed, important.

The determination of the number of oysters, though as a matter of course approximate only, is important as giving a point of comparison by which future decrease may be measured by repeating the investigation in similar fashion.

There is no doubt that in a comparatively limited time the majority of the Chesapeake beds will be practically destroyed, so far as producing oysters for a market is concerned. Some forty thousand people will have to seek employment in a different field. Probably, under the circumstances, this is the best thing that could happen; for it is doubtful if any less drastic medicine would have the slightest effect on the population residing in the vicinity of the oyster-beds, who, in the face of all the facts, have persisted in setting themselves like flint against any modification or check on their career of destruction. The present observations on the growth and surviving percentage of young oysters on the tile-collectors would have been much fuller and more valuable, had not the oystermen cut the buoys adrift, stolen the thermometers and lines, and destroyed such collectors as they could reach unseen, with the stupid notion that some reservation of beds, or limitation of fishing, was to result from the investigation. Twenty-four bundles of tiles were set and buoyed between July 1 and 14, and by Aug. 1 all but one were

FIG. 2.—LOWER SIDE OF TILE EXPOSED JULY 9–AUG. 23. (TWO-THIRDS NATURAL SIZE.)

removed or destroyed. Fig. 1 represents a portion of one of these tiles, which was placed in position July 9. On July 19, when first

examined, there were a few oysters upon it, but so small that a microscope was necessary to recognize them. On Aug. 2 it was again examined, and the tile of which a portion is figured was removed from the bundle. There were then from 26 to 348 young oysters on a tile; the total number upon the whole bundle was 1,506.

The third examination was made Aug. 23, when it was found the oysters had increased very much in size and numbers. On the tiles remaining, there were 1,334 oysters. A tile of which a portion is represented in fig. 2 was then removed. On Oct. 10 the bundle was again examined. The oysters had decreased fifty-five per cent in numbers; but two-thirds of them were now over three-quarters of an inch, and two specimens over two inches long, though the shells were still extremely delicate. Part of one of these tiles is represented by fig. 3.

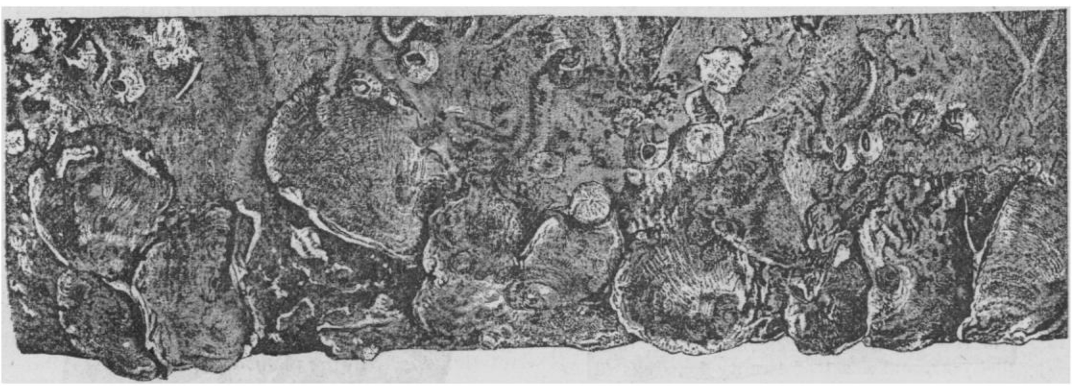


FIG. 3.—UPPER SIDE OF TILE EXPOSED JULY 9–OCT. 10. (TWO-THIRDS NATURAL SIZE.)

It was thus determined, that in 1879 the attachment of the young oysters began about the middle of July, and continued about a month, as after Aug. 20 there were no signs of fresh attachments; that fully fifty per cent died from natural causes within six weeks, no traces of predacious mollusks being noticed on the dead shells, though the evidence on this point is imperfect; that, the attachments being far more profuse on the concave under side of the tiles, the spat just previously must be on or near the bottom, and must rise to attach themselves; lastly, that the rate of growth is much more rapid than had previously been supposed, and may reach two inches in length in three months. Numerous other points of interest may be gleaned from the report, for which we have not space. Enough has been said, however, to show the value of Lieut. Winslow's work, the intelligence and assiduity with which it was carried on, and the wide field which awaits further investigation.

THE PEBBLES OF SCHLESWIG-HOLSTEIN.

Die sedimentär-geschiebe des provinz Schleswig-Holstein. Von Dr. C. GOTTSCHE. Als manuscript gedruckt. Yokohama, *Lévy & Salabelle*, 1883. 6+66 p., 2 maps. 8°.

THIS treatise by Dr. Gottsche, who is at present in Yedo, was an accepted thesis for admission to the position of private teacher at the Kiel university in 1880, printed privately in German at Yokohama in 1883, and seems to be a very painstaking and pretty thorough description of the pebbles, whether of rocks, minerals, or fossils (seventy-six kinds in all), found in four quaternary sedimentary beds at Kiel, with especial reference to the identification of their source, and is accompanied by two maps, — one showing with straight lines thirty directions in which such pebbles of the lowest bed appear to have been transported, and the other giving with similar lines the dissemination of three particular kinds of rock in the same Baltic region. Many of the lines are only a couple of hundred miles long, but some are six hundred or more. The author himself points out that the pebbles have not by any means necessarily been carried along those straight lines; and the place of origin may not necessarily have been exactly at the points where identical rocks are only found at present. Nevertheless the lines show that the transfer has in general been from the north-east, north, or north-west, and never from the westward or southward of Kiel. Of course,

there need have been no more than two directions of movement, south-westerly and south-easterly; for the pebbles carried a part of their course in one direction may have been carried the rest of the way in the other, and so produced any resultant direction between the two; or materials carried by floating ice may have come in a far more crooked course (and the places of origin are all on the shores of the Baltic, or on streams flowing into it). The lower sedimentary bed, with only a couple of exceptions, contains, so far as now known, every kind of pebble found in the upper ones, so that no inferences can yet be drawn as to changes with time in the direction of transport. The main result would seem then to be, that the Kiel sediments have all come from more northern parts of the Baltic basin, and might have been carried chiefly by floating ice, without a climate so very different from the present one.

The author is highly to be commended for his liberality in printing his pamphlet of sixty-six large octavo pages at his own expense, and that, too, in a country where good European printing is particularly troublesome. The two maps might, perhaps, have been advantageously combined in one, if one of the two sets of lines had been of a different character (say, dotted or broken) or of another color; for the very object of cartographic representation is to show at one view as much as can possibly be distinguished clearly of any given subject,—to assemble for convenient comparison on one sheet as many as may be of the scattered facts of nature bearing upon any given point.

WEEKLY SUMMARY OF THE PROGRESS OF SCIENCE.

ASTRONOMY.

Spectra of comets observed in 1881.—P. Tacchini discusses the varying appearances presented by the spectra of the comets *b* and *c* 1881, and accompanies his remarks with an extensive series of nearly forty lithographed drawings illustrating the changes which occurred. These changes, for the most part, consist merely in variations of the brightness and diffusion of the observed bands, and not in any alterations of position. He gives also a single figure of the spectrum of Encke's comet, observed the same year, and a set of twenty drawings of the comets (*b* and *c*) themselves. The paper, with its accompanying plates, constitutes an important collection of observed data; and some slight discrepancies between these representations and those of other observers raise interesting questions.—(*Mem. soc. spettr. ital.*) C. A. Y. [263

Uranus.—Within the last few months, considerable attention has been paid to this planet, and a number of series of observations upon it have been published. Safarik (*Astr. nachr.*, 2505), Meyer (*Astr. nachr.*, 2524), and Schiaparelli (*Astr. nachr.*, 2526), all present the results of their measures made for the purpose of determining its diameter and ellipticity. The observations of Schiaparelli are the most numerous and complete. He finds for the equatorial diameter of the planet 3″.911, and, for the polar, 3″.555 (both reduced to the mean distance 19.1826). This gives the ellipticity of the planet $\frac{1}{11}$, nearly the same as that of Saturn. He also reports the existence, upon the planet's disk, of spots and changes of color, too faint, however, to admit of delineation by means of a telescope of only eight inches aperture. In fact, to have seen them at all with such an instrument is a most remarkable evidence of the wonderful clearness of the Italian sky. The writer of this notice also made a series of observations upon the same object, in May and June, with the twenty-three inch equatorial of the Princeton observatory. Markings upon the planet's disk were unmistakably visible as belts resembling those of Jupiter and Saturn. The equatorial diameter determined by the writer's measures is 4″.280, and the polar, 3″.974, giving an ellipticity of $\frac{1}{14}$. Mädler, in 1843, obtained 4″.304 and 3″.869 for the two diameters, and an ellipticity of $\frac{1}{10}$. There can no longer be any doubt that the planet has a rapid rotation nearly in the plane of the satellite-orbits.—C. A. Y. [264

MATHEMATICS.

Perimeter of the ellipse.—Mr. Thomas Muir, referring to a recent article by M. Mansion, infers that the following formula, which he has known for some time, for calculating approximately the perimeter of an ellipse, has not yet been published. Denoting as usual by *a* and *b* the semi-axes of the ellipse, the expression for the perimeter is

$$2\pi \sqrt[\frac{3}{2}]{\frac{a^{\frac{3}{2}} + b^{\frac{3}{2}}}{2}};$$

or, *the perimeter of an ellipse is approximately equal to the perimeter of a circle whose radius is the semi-cubic mean between the semi-axes of the ellipse.*—(*Mess. math.*, xii. no. 10.) T. C. [265

Calculus of variations.—The general problem of the calculus of variations is to find the variation of an *n*-tuple integral of a function of *n* independent variables, and of depending also upon a number of arbitrary functions of these variables, together with the differential coefficients of the functions. M. Picart in his paper, which he entitles *Theorie nouvelle du calcul des variations*, confines his attention to a triple integral containing only one arbitrary

function, and solves several of the more fundamental problems connected with the determination under various conditions of the variation of the integral. In particular, he shows how the problem of relative maxima or minima can be conducted to that of absolute maxima or minima. — (*Nouv. ann. math.*, Feb.) T. C. [266

PHYSICS.

(*Photography.*)

Photographing Reichenbach's flames. — The question of the actual existence of these flames, surrounding the poles of powerful magnets, has again been brought up for discussion in scientific circles. Numerous persons have claimed to be able to see them, and some even to be able to distinguish between the poles and the color of the flames. Reichenbach himself attempted to photograph them by the daguerrotype process, but was apparently dissatisfied with the results he obtained. Mr. William Brooks has taken the matter up, and thinks he has obtained actual impressions of the flames, by means of photography, on sensitive dry plates prepared especially for the purpose. In total darkness a perforated blackened card was placed one-eighth of an inch above the poles of a permanent horseshoe-magnet, and a sensitive plate placed an eighth of an inch above the card. With five minutes exposure he obtained a result; and this was repeated many times, the most remarkable thing being, that sometimes he obtained a positive and sometimes a negative image, under precisely the same conditions. Another curious effect obtained was, that some printed matter, which was under the wash of Indian ink used to blacken the card, was perfectly readable when the plate was developed. This latter result, however, was obtained on only one occasion. He also succeeded in obtaining prints through a glass plate on which were painted figures in black varnish. This was contrary to the experience of Reichenbach, who considered that the rays were not transmitted through glass. — W. H. P. [267

Hydrokinone. — Of this new developer, first introduced by Capt. Abney, Mr. Charles Ehrmann says, "The best results I have obtained with ten grains of hydrokinone to eight ounces of water, and caustic ammonia (1 to 7) added gradually as the development progressed. The negatives are of a non-actinic color, similar in tone to one slightly intensified with uranium and prussiate of potash; therefore the development need not be carried on very far, thus preserving all finer modulations. An injudicious amount of alkali will produce green fog." — (*Phot. times*, July.)

Of this same developer, Mr. Edwin Banks claims that it is much more powerful than pyro, and that it will bring out a fully developed picture with at least half the exposure that is necessary when pyro is employed. At first sight this seems strange, when it is observed how much more powerfully the latter absorbs oxygen; but the explanation probably lies in the fact that hydrokinone is more gradual in its action, and has a greater selective power, than pyro. With a collodio-bromide film, for instance, which is not so much protected from chemical action as one of gelatine, pyrogallic acts with such energy when mixed with an alkali, that the whole film is reduced immediately, and no image, or only a faint one enveloped in fog, appears: hence a powerful restrainer must be used to keep this action within bounds. A soluble bromide, which is the salt commonly used, has this effect, but, unfortunately, at the same time partially undoes the work which the light has done, rendering it necessary to give a longer exposure. But with hydrokinone no restrainer is necessary, unless a great error in exposure has been made. It does its work rapidly and cleanly, in this respect resembling ferrous oxalate. It does not discolor during development so much as pyro, and consequently does not stain the film so much, whilst full printing vigor is very easily obtained without having to resort to intensification. The color and general appearance of the negative are more like those of a wet plate, since the shadows remain quite clear, and free from fog. It seems almost impossible to fog a plate with it. One grain of hydrokinone to the ounce is strong enough for most purposes. With some samples of hard gelatine it is advisable to use two; but with most kinds and with collodion, one grain is sufficient. Two or three drops of a saturated solution of washing-soda to the ounce of the hydrokinone solution rapidly develops the image, and the addition of a few drops more to complete development is all that is needed. A soluble bromide acts very powerfully as a retarder and restrainer. With a mere trace added, development is very much slower. — (*Brit. journ. phot.*, July 6.) W. H. P. [268

ENGINEERING.

Sources of error in spirit-levelling. — Precise levelling in this country has been done by the U. S. lake survey, which has determined the elevation of all the great lakes with a probable error of less than one foot; by the coast and geodetic survey, which is carrying a line of levels across the continent from Chesapeake Bay to San Francisco; and by the Mississippi River commission, which has a line from the Gulf as far north as central Iowa, to be connected with Lake Michigan, and thence with the sea-level at New York. Mr. J. B. Johnson has been connected with some nine hundred miles of this work, and discusses the sources of error. He first classifies errors into compensating and cumulative. Then he treats them as, 1°, errors of observation, in the instrument or in the rod; 2°, errors from instrumental adjustment; 3°, errors from unstable supports; 4°, atmospheric errors, from wind, from tremulousness of the air caused by difference of temperature, and from variable refraction. He concludes, that, with good instruments and proper care, thirty miles of line should be duplicated a month with one Y-level and a target-rod, and all discrepancies brought within five-hundredths of a foot into the square root of the distance in miles; or with the U. S. precise levels and speaking-rods, reading three horizontal wires, one instrument should bring the discrepancies within two-hundredths of a foot into the square root of the distance in miles.

The Mississippi River levels have been well within this limit. — (*Journn. assoc. eng. soc.*, March.) C. E. G. [269

GEOLOGY.

Lithology.

Gold in limestone. — According to Prof. C. A. Schaeffer, gold occurs in a ferruginous cretaceous limestone from Williamson county, Tex. This rock lies near the surface, and fifty-two samples procured *in situ* by him averaged $15.20. Twenty contained no gold, while thirty-two assayed from $1.00 up to $231.50 per ton. He regards the gold as having originally existed in the limestone in pyrite, which has since been removed and the gold locally concentrated. — (*Trans. Amer. inst. min. eng., Boston meeting.*) M. E. W. [270

The Ottendorf basalt. — Rudolf Scharizer discusses the occurrence, microscopic and chemical composition, of this Silesian (Austria) basalt, its alteration, and contact phenomena with the grauwake-sandstone. The paper is quite full of chemical analyses. Olivine, somewhat serpentinized, is the predominating mineral, enclosed in a ground-mass of augite, magnetite, biotite, anorthite, nepheline, etc. The chemical analysis indicates that the rock is closely allied to the peridotites, if it does not belong to them. — (*Jahrb. geol. reich.*, xxxii. 471.)

The same journal contains an extended paper by Messrs. Teller and John, on the geological and lithological characters of the dioritic rocks of Klausen in the South Tyrol, a series of very diverse rocks including gabbros or norites. — (*Ibid.*, 589.) M. E. W. [271

METEOROLOGY.

The rain-storm in Ontario on July 10. — The Canadian meteorological service has made a special investigation of this storm, which caused such unusual destruction in the vicinity of London, Ontario. Observations from over one hundred observers were received and studied. The isobaric curves show only such undulations as generally accompany showers and thunder-storms in the summer season; and there was nothing in the maps to warrant the expectation of any storm, beyond the 'local showers' which were officially predicted, and which occurred in other parts of Ontario. The fluctuations in barometric pressure were hardly appreciable, and there was but little wind. Indeed, the only peculiarity of the storm was the unaccountable and unexpected precipitation, which exceeded four inches where the maximum occurred. This amount was recorded in an elliptical area of country, extending in a direction about north-west and south-east, and covering a territory of about twenty by fifty miles. The devastation at London was due to the fact that the two branches of the Thames River, which there unite, approach from nearly contrary directions, the river flowing away nearly at right angles to the branches. The question is therefore raised, whether it would not be advisable to divert one of the branches, that it may meet the other at an acute angle, and thus lessen the probability of a repetition of the catastrophe. The need of an increased number of rainfall observers is pointed out, that means may be afforded for extensive study into the little-known subject of the course and causes of local rains. — (*Can. weath. rev.*, July.) W. U. [272

GEOGRAPHY.

(*Alpina.*)

Ascent of Indrapura, Sumatra. — An account is recently published of the persevering and first successful attempt of Veth and Van Hasselt, several years ago (1877), to ascend this highest of the Sumatran volcanoes. They had to choose a way through the dense forest of the lower slopes, and over the sharp, loose rocks nearer the summit; and sudden heavy rains caused them much delay, so that eight days were spent in reaching the highest point, although the rim of the crater was gained a day earlier. Elephant-tracks were not found above 1,500 met., rhinoceros-tracks not above 2,600; but wild goats had been on the very summit. Above 2,500 met., large trees were absent; and above 3,000 only a few plants had found place to grow on the naked volcanic rocks. The barometer read 482.4 mm., and the thermometer, 8° C., corresponding to a height of about 3,700 metres. The surrounding country had the appearance of a uniform forest wilderness, occasionally broken by volcanic peaks and ranges, and showing a cultivated region by its lighter color in the distance near the coast. A deep crater lay within the sharp, ragged walls; several streams ran down to a pool at the bottom, a thousand metres below the rim, whence sulphurous vapors and clouds of steam rose into the great caldron. The volcano was in eruption in 1842, when described by Junghuhn. The descent was accomplished without serious difficulties. — (*Deutsche geogr. blätter*, vi. 1883, 130.) W. M. D. [273

(*South America.*)

Bolivian rivers. — On the occasion of Dr. E. R. Heath's account of his exploration of the Beni and other rivers flowing from the Andes north-eastward to the Amazon system, Mr. C. R. Markham, secretary of the Royal geographical society, gave a general description of the region, part of which he had visited in 1853. The mountains in which the rivers rise are part of the eastern range of the Andes, rising into great peaks like Illimani and Illampu, to a height exceeding 21,000 feet, with fossiliferous silurian rocks up to their summits. To the west is the great interior plateau of the Titicaca basin; to the east, the rivers descend, bearing gold gravels to the great plains, covered with unbroken forest. This eastern region has been very little explored; and the india-rubber and cinchona bark gathered about the upper streams are carried westward over the mountains to the Pacific ports, rather than down the rivers to the Amazon and the Atlantic. Markham sketches the history of exploration here from the time of the Inca expedition in the fifteenth century to the expeditions of Maldonado down the Amarumayu in 1866, and Heath down the Beni in 1880; these being the only travellers who have followed the rivers down to their junction. Dr. Heath mapped the whole course

of the Beni with great care, taking astronomical observations, and measuring the width, depth, and velocity of the water. On reaching the Madeira, as the river is called below the junction of the Beni and Mamoré, he ascended the latter to Exaltacion, and then followed Yacuma, and crossed the country beyond its source to Reyes again on the Beni, where his return was celebrated by a public reception and a special mass. The people here were greatly excited over his report on the number of rubber-trees in the country he had passed through; and from 185 men engaged in collecting 104,000 pounds of rubber in 1880, the number increased to 644 in four months, and must now reach one or two thousand. From Reyes he ascended the Beni to La Paz. His report is very brief, and contains little beyond an itinerary; rapids and rocks are occasionally mentioned, and a few lakes were passed, but there is no material given toward a physical description of the country.—(*Proc. roy. geogr. soc.*, v. 1883, 313, map. [Dr. Heath's paper is also given in the *Bull. Amer. geogr. soc.*, 1882, no. 3.]) W. M. D. [274

BOTANY.

American smuts. — Farlow, in some notes on Ustilagineae, gives the first account of American Entylomata, his list including eight species, one only of which appears under another genus in earlier lists. Four of these are, for the present, described as new, though two may prove to be identical with species growing on the same host genera, in other countries. One is doubtfully considered to be a form of a European species; the balance occur also in the old world. Two American species of Cornu's new genus Doassansia — D. Farlowii Cornu and D. epilobii Farlow — are recorded; the former in the ovaries of Potamogeton, the latter in leaves of Epilobium. — (*Bot. gazette*, Aug.) W. T. [275

Fertilization of Leptospermum. — In a fourth paper on the indigenous plants of Sydney, E. Haviland considers the structure of the reproductive organs of this genus and its mode of fertilization. Cross-fertilization is regarded as probably the rule, brought about, 1°, by the difference in the times of maturing of the anthers and stigma; and, 2°, by changes in their relative positions. — (*Linn. soc. N. S. Wales; meeting* June 27.) [276

ZOÖLOGY.

Mollusks.

Astarte triquetra Conrad. — This minute and peculiar shell, recently rediscovered by Mr. Hemphill in Florida, but described by Conrad more than thirty years ago, proves to be a new form, Callicistronia, perhaps related to Tivela, with a small sinus in the pallial line, two large cardinal teeth in one valve, and one in the other. It is viviparous. More than fifty young ones were found in a single specimen, recalling the habit of Psephis. — W. H. D. [277

Anatomy of Urocyclus. — Dr. Paul Fischer has examined the soft parts of Urocyclus longicauda F. from Madagascar. The digestive tract resembles that of Parmacella and Limax. There is a large mucus-vesicle analogous to the vestibular prostate in Parmacella, Tennentia, and Ariophanta. Otherwise the reproductive organs resemble those of Helicarion, and a slug described in detail by Keferstein under the name of Parmarion in 1866, and which proves to be a true Urocyclus. This genus is African, while Parmarion is of Asiatic and East-Indian distribution. Urocyclus has an oxygnathous arcuate jaw, a rhachidian, thirty-nine lateral and thirteen uncinal teeth in one hundred and twenty-five rows. Dendrolimax of Heynemann appears to differ from Urocyclus merely in the absence of the mucus-vesicle, and will fall into synonymy. — (*Journ. de conchyl.*, xxii. 4.) W. H. D. [278

VERTEBRATES.

Reptiles.

Nerve-endings in the caudal skin of tadpoles. — The epidermis of the skin of tadpoles has two layers of cells. In the deeper cells, on the tail, appear peculiar bodies, first seen by Eberth (*Arch. mikros. anat.*, ii. 90) and Leydig (*Fortschr. naturf. ges. Halle*, 1879, taf. ix. fig. 32). The latter compared the bodies in question with the nettles of lasso-cells, giving to the cells containing the bodies the strange name of 'byssuszellen.' Pfitzner (*Morph. jahrb.* vii. 727) showed that these bodies are united with nerve-filaments, every one of the cells being so supplied. The nerves of the skin had been studied by Eberth (*l.c.*) and Hensen (*Virchow's arch.*, xxxi. 51; *Arch. mikros. anat.*, iv., 11). Canini and Gaule have studied the subject afresh, rectifying and supplementing the previous writers. The bodies in the basal epidermal cells appear as thick rods curved into bizarre and varying shapes. Each is connected with a nerve-filament (sometimes, but not always, as maintained by Pfitzner, two filaments run to one cell). The filaments descend through the gelatinous corium (cutis), to unite just below with a thick nucleated network of threads, which, from their reactions, are regarded as nervous tissue, and distinct from the cojacent plexus of connective-tissue corpuscles. This network, again, is connected with a deeper-lying, coarser plexus, corresponding to Ranvier's *plexus fondamentale.* These peculiar end-organs are not found, except in the tail: they are probably sensory, but Gaule hesitates to deny Leydig's interpretation. — (*Arch. anat. physiol., physiol. abth.*, 1883, 149.) C. S. M. [279

Birds.

Xenicidae, a new family. — On dissection of a specimen of Xenicus longipes and one of Acanthositta chloris, Mr. Forbes found the syrinx to be strictly mesomyodian. On account of this, the long tenth primary and the non-bilaminate tarsus, the birds are removed from the vicinity of Sitta as a family, Xenicidae, of non-oscinine Passeres in the vicinity of the Pittidae. — (*Proc. zoöl. soc. Lond.*, 1882, 569.) J. A. J. [280

Anatomy of the todies. — After a careful examination of the structure of this group, Mr. Forbes concludes that the todies are an isolated form of anomalogonatous birds, with no clear affinity to any living group. He therefore proposes to raise them to the group Todiformes, equivalent to the Passeri, or Piciformes.—(*Proc. zoöl. soc. Lond.*, 1882, 443.) J. A. J. [281

Illinois birds. — Nehrling continues his annotated list of Illinois birds in the full and learned manner so distinctive of German work. The present instalment contains thirty-nine species, from the bobolink to the great horned owl inclusive.—(*Journ. f. ornith.*, xxxi. 84.) J. A. J. [282

Mammals.

The os intermedium of the foot. — Dr. Karl Bardeleben gives a *résumé* of his observations upon the bones of the foot. A well-developed intermedium is present in many species of marsupials, but not in all. Its presence in a given species does not always imply its existence in closely allied species. For example: it occurs in Chironectes variegatus, but not in C. palmatus. The bone varies in size from one centimetre to a fraction of a millimetre. It does not exist in marsupials of which the hand has undergone regressive alterations, e.g., Halmaturus Bennetti, H. giganteus, etc. The separation of an intermedium is indicated in the monotremes, many edentates, as well as in the genera Elephas, Hippopotamus, and Tapirus, by a fissure, more or less deep, in the astragalus. Dr. Bardeleben suggests the name 'os trigonum' for the bone in question. — (*Zool. anz.*, no. 139.) F. W. T. [283

Odontoblasts and dentine. — R. R. Andrews has studied the development of teeth in pig embryos, and publishes the remarkable conclusion that the odontoblasts entirely disappear, forming the matrix of the dentine, and have nothing to do with the dentinal fibrils, which he claims arise from deeper layers, probably from nerve-fibres. (We are not prepared to agree with these views.) — (*N. E. journal of dentistry*, ii. 193.) C. S. M. [284

(*Man.*)

Measurements of the depth of sleep. — Two of Vierordt's pupils, Mönninghoff and Piesbergen, have made the depth of sleep the subject of an investigation. They worked upon the principle that the depth of sleep is proportional to the strength of the sensory stimulus necessary to awaken the sleeper, that is, to call forth some decisive sign of awakened consciousness. As a sensory stimulus they made use of the auditory sensation produced by dropping a lead ball from a given height. The strength of the stimulus was reckoned, in accordance with some recent investigations of Vierordt, as increasing, not directly as the height, but as the 0.59 power of the height. For a perfectly healthy man, the curve which they give shows that for the first hour the slumber is very light; after 1 hour and 15 minutes, the depth of sleep increases rapidly, and reaches its maximum point at 1 hour and 45 minutes; the curve then falls quickly to about 2 hours 15 minutes, and afterwards more gradually. At about 4 hours 30 minutes, there is a second small rise which reaches its maximum at 5 hours 30 minutes, after which the curve again gradually approaches the base line until the time of awakening. Experiments made upon persons not perfectly healthy, or after having made some exertion, gave curves of a different form. — (*Zeitsch. f. biol.*, xix. 114.) W. H. H. [285

ANTHROPOLOGY.

Notes on Mitla. — In July, 1881, Mr. Louis H. Aymé visited the ruins of Mitla, which lie in Oaxaca directly south of Vera Cruz. Mitla is not so grand, so magnificent, as Uxmal; but it has a beauty of its own, as it nestles quietly at the foot of the mighty mountains, the ruins of grim 'Fortin' standing sharp against the evening sky; and, as the sun sinks, one might fancy he heard the weird chant of the priests, the lament of the mourners for the dead who rest in Lyobaa, the Centre of Rest. Appended to M. Aymé's itinerary is a translation by Mr. S. Salisbury, jun., of the description of Mitla, by Francisco de Burgoa, written in 1674. Then follows a report of the various buildings constituting the north and south groups, which for detailed statement and brevity is a model archeological document. Mr. Aymé is able to correct some of the errors of his predecessors. It is gratifying to quote the following: "The buildings are carefully looked after by the government, and have an intelligent guardian in the person of Don Felix Juero." Comparing the present account with Burgoa's, Mr. Aymé concludes that in 1644 the ruins were practically as they are to-day. — (*Proc. Amer. antiq. soc.*, ii. 82.) J. W. P. [286

The Olmecas and the Tultecas. — Mr. Philipp J. J. Valentini gives some very cogent reasons for thinking that the sanguine hopes of the decipherers of American hieroglyphics will never meet the realization of those who unravelled the sacred languages of Egypt and Mesopotamia. Except for the wonderful similarity which early Mexican civilization bears to that of the ancient nations of the eastern hemisphere, only a fraction of the workers could have been induced to undertake the labor. The right way to treat these matters is to moderate our expectations. With such motive, the author then endeavors to fix the main epochs, and to inquire who were the Olmecas and the Tultecas. The former search results in fixing the dates of all we know concerning Mexican history between the years 232 and 1521 of the Christian era. Mexican history begins with the record of a race of giants, the Quinamé, or Quinametin, who are claimed to have been a people of Maya origin, found by the Nahuatls on the Atoyac River, when they were migrating southward. When the name of the Olmecas appears in the early Mexican records of the Nahoas, we must not hesitate to recognize in them that people east of Anahuac who spread along the Atlantic slopes and south through Yucatan, Tabasco, and the whole of Guatemala, and whom we designate to-day by the collective name of Maya. No nation, empire, or language of Tultecas ever existed. The Tultec exodus is shown to refer to the migrations of the Colhuas who shared with the Mexicans the rule of the uplands. Their journey to Culiacan was not from the Pueblos, but from the borders of the Gulf of Mexico. — (*Proc. Amer. antiq. soc.*, ii. 193–230.) J. W. P. [287

North-eastern Borneo and the Sulu Islands. — Although north-eastern Borneo is close to the Sulu group, there is a great difference in the people. The Sulus are Malays, with a considerable infusion of Arab and Chinese blood. The Bajaws, or sea-gypsies,

lead a nomadic life in their boats, each boat containing an entire household. The Sulus are divided into coast Sulus and the Orang Gumber, living among the hills, and they are much above the Bajaws in character. The latter are stronger in physique, but timid and treacherous. On the coast-line of Borneo is an extraordinary mixture. At Melapi, sixty miles up the Kina Batangan, are Sundyaks, Malays, Javanese, Sulus, Bajaws, Bugis, Chinese, Arabs, Klings, and many others ; while of the Buludupies, the indigenous inhabitants, there are hardly any of pure blood left. These indigenes are an interesting people, their ancestry showing distinct signs of a Caucasian type. The rest of north-eastern Borneo is inhabited by tribes of the race styled Eriaans, Dusuns, or Sundyaks, who are of Dyak blood, with perhaps an infusion of Chinese. The Chinese language, dress, etc., are entirely lost, however. Slavery of a clan or feudal type is universal, and the Mohammedan religion prevails. The Sundyaks are divided into many tribes, some of which are gaining in power. *Cf.* i. **552.** — (*Proc. roy. geogr. soc.*, v. 90.) J. W. P. [288

INTELLIGENCE FROM AMERICAN SCIENTIFIC STATIONS.

PUBLIC AND PRIVATE INSTITUTIONS.

Dudley observatory, Albany, N.Y.

Comet b, 1883 (Brooks). — By means of observations secured at the Dudley observatory on Sept. 5, 9, and 18, I derived on the 19th the following parabolic elements, marked I. The remarkable similarity of these elements to those given by Schulhof and Bossert for the Pons comet of 1812 pointed unmistakably to their identity. The elliptic elements of the Pons comet (here marked II.) are transcribed from the memoir of Schulhof and Bossert (p. 150), except that they are reduced to the mean ecliptic and equinox of 1883.0, and a value of T, derived from observations of the present apparition, is substituted.

I.		II.	
T=1884, Jan., 25.788 (G.M.T.).		T=1884, Jan., 25.696 (G.M.T.).	
Node	254° 13′.6	Node	254° 8′.8
Node to perihelion .	199 14 .4	Node to perihelion .	199 12 .9
Inclination	74 47 .1	Inclination	74 03 .3
Log. q.	9.87944	Log. q.	9.88930
		Eccentricity . . .	0.95527

The value of T in II. was determined by approximation from the observation of Sept. 5. The remaining observations do not indicate any important change in its value. The following ephemeris results from elements II. The geocentric positions are referred to the mean equinox of 1883.0.

Greenwich, 12 hours.	α	δ	Log. Δ	Light.
	h. m. s.	° ′		
Sept. 2	16 36 37	65 03.0	0.3725	.03
" 6	32 19	64 13.9	0.3648	.03
" 10	29 06	63 23.0	0.3569	.04
" 14	26 53	62 31.1	0.3487	.04
" 18	25 37	61 38.3	0.3400	.04
" 22	25 15	60 45.2	0.3310	.05
" 26	25 45	59 52.4	0.3215	.05
" 30	27 01	58 59.6	0.3115	.06
Oct. 4	29 06	58 07.5	0.3009	.06
" 8	31 57	57 16.5	0.2897	.07
" 12	35 32	56 26.5	0.2779	.08
" 16	39 52	55 37.6	0.2653	.08
" 20	44 56	54 49.9	0.2518	.09
" 24	50 47	54 03.3	0.2377	.10
" 28	57 25	53 17.8	0.2226	.12
Nov. 1	17 04 53	52 33.3	0.2065	.14
" 5	13 15	51 49.6	0.1893	.16
" 9	22 34	51 06.0	0.1708	.19
" 13	32 56	50 22.4	0.1512	.22
" 17	44 26	49 37.0	0.1302	.26
" 21	57 14	48 49.4	0.1077	.32
" 25	18 11 27	47 57.1	0.0836	.38
" 29	27 16	46 58.1	0.0580	.46
Dec. 3	44 50	45 43.2	0.0300	.57

In the light scale, .19 corresponds to that of discovery in 1812, and 1.00 to the time when the comet was reported as visible to the naked eye in the apparition of 1812. The places of the above ephemeris represent the observations already made within about 30″ in each co-ordinate, and with a very uniform minus value of 'c-o' throughout. This seems to be the fault of the elliptic elements. Any considerable change in the time of perihelion passage diminishes the discrepancy in one co-ordinate at the expense of the other.

It is remarkable that the present comet should have been picked up when its light ratio was six times as small as it was at discovery, in 1812. It was then regarded as a faint object. Were it not for the overwhelming testimony from other sources, one might doubt, on the ground of brightness, the identity between the present comet and that of 1812. The following rough ephemeris may be of interest: —

	α	δ	Light.		α	δ	Light.
1883.	h. m.	°		1884.	h. m.	°	
Dec. 3	18 45	+ 45.7	.6	Feb. 1	0 34	− 28.3	2.3
" 13	19 37	+ 41.7	1.0	" 11	1 02	− 37.2	1.5
" 23	20 41	+ 33.9	1.8	" 21	1 23	− 43.7	1.0
1884.							
Jan. 2	21 53	+ 22.1	3.5	Mar. 2	1 43	− 48.5	.6
" 12	23 01	+ 2.5	4.1	" 12	2 02	− 53.0	.4
" 22	23 53	− 15.2	3.0	" 22	2 26	− 56.2	.4

The identity of the Pons comet of 1812 with comet b, 1883, was announced in an 'associated press' despatch from the Dudley observatory on the evening of Sept. 19. LEWIS BOSS.

Sept. 21, 1883.

Massachusetts institute of technology, Boston, Mass.

Extension of the course in biology. — Advantage is at once to be taken of the extension in the building accommodations, and the improvement in the financial resources of the institute, to greatly enlarge the space heretofore given to biological work, and to increase the instructing staff of this department of the school.

The removal of the physical laboratory to the new building on Clarendon Street affords the long-desired opportunity for the expansion of the biological laboratory, heretofore confined to a single small room in the

low brick annex. The large north room (90 x 28 ft.) on the first floor of the main institute building (the Rogers Building), with its admirable light and its many facilities, will be devoted to the purposes of the natural-history course, and will be fitted up with appropriate apparatus and instruments. Within a short time, it is also anticipated that a room in the basement (being one of those now occupied by the chemical or by the metallurgical department) will be available for use in dissections and in the coarser work of a biological laboratory.

Dr. W. T. Sedgwick, a graduate of the Sheffield scientific school, and recently connected with the biological department of the Johns Hopkins university, having been appointed assistant professor of biology, will assume charge of the biological laboratory at the opening of the next school year, and will give the instruction in physiology, botany, and general biology, now provided for in the regular courses of the institute, especially in the so-called natural-history course, as well as take charge of the work of special students in these branches.

The instruction given in geology by Professor Niles, and in zoölogy and paleontology by Professor Hyatt, will be continued. Mr. W. O. Crosby has been appointed assistant professor of mineralogy and lithology, and will hereafter give, throughout the school year, the instruction which has heretofore been confined to a single term. The advantages of the extension of the chemical and physical laboratories, abundantly provided for in the new building of the institute, will be enjoyed by the students of the natural-history course, in common with those of the other regular courses.

In view of the foregoing enlargement of facilities and opportunities for study and research in the branches especially embraced in this course, it is recommended to students looking forward either to becoming naturalists, or to the subsequent study and practice of medicine.

NOTES AND NEWS.

The comet recently detected by W. R. Brooks at Phelps, N.Y., has become an object of unusual interest since its identification with the comet of 1812, the return of which has been anticipated about this time. Mr. Brooks first noticed the comet as a suspicious object on the night of Sept. 1, and directed the attention of astronomers to it, after a second observation. During the first half of September it was repeatedly observed at various places; but its great distance and consequently slow movement made it difficult to obtain trustworthy approximations to its orbit, and thus delayed the recognition of its character. Its identity with the comet of 1812 was first announced, so far as we are informed at present, by the Rev. George M. Searle of New York, in a letter published on Sept. 18. A communication from him to Harvard college observatory, with which he was formerly connected, was received there on the morning of Sept. 20, and contained a statement of the process by which he reached the interesting conclusion previously announced. This consisted in determining, from the positions of the Brooks comet, the corresponding points of intersection with Encke's orbit of 1812; the result for the time of perihelion passage being 1884, Jan., 25.17, and the longitude of the perihelion being closely accordant with that given by Encke.

Professor Boss of the Dudley observatory, as will be seen on an earlier page, arrived independently at the same conclusion by computing parabolic elements from observations of Sept. 5, 9, and 18, which exhibited a close similarity with those of the orbit of 1812. The circular which he has issued upon the subject states that he communicated his result to the associated press on the evening of Sept. 19.

The communication of Father Searle to Harvard college observatory, already mentioned, induced Mr. Chandler to examine the question, with the aid of the most recent observations. The result was to furnish further confirmation of the asserted identity; and the positions obtained at the observatory as late as Sept. 22 make it still more evident. The difference between the observed place and that resulting from the orbit of 1812, as corrected by the recent publication of Schulhof and Bossert, but with the time of perihelion passage assumed as 1884, Jan., 25.780, is as follows: —

Diff. R.A. $-0^s.1$. (O—C.)
" Decl. $+66''$.

This agreement is entirely within the uncertainty of the orbit of 1812, from the old observations.

The comet has also exhibited phenomena of great interest in regard to the development of its structure by its approach to the sun. When first observed this year, it was a very faint and small nebulous object, but the appearance of a stellar nucleus was noted at Harvard college observatory by Mr. Wendell on Sept. 3. The nucleus was afterwards less distinct. This may have been due to unfavorable conditions of observation, or it may possibly indicate a preliminary series of changes like those which the comet has just exhibited. On Sept. 21, as seen at Harvard college observatory, the comet was still very faint. A slight condensation at one place could be seen with the large equatorial, but this could hardly be called stellar. The next night, Sept. 22, the appearance of the comet had so completely changed that it was difficult to believe it the same object previously seen. It now resembled a star nearly as bright as one of the eighth magnitude. Very little nebulosity could be detected about it, but some was seen early in the evening, while the comet was sufficiently high in the sky. During the evening it appeared to be gaining perceptibly in brightness. The next night, Sept. 23, it was seen at times between clouds, and was found to have again changed its appearance. It was now even brighter than before (although still slightly inferior to a star of the eighth magnitude), but it had lost its stellar appearance, and had become blurred, regaining the ordinary character of a cometic nucleus. Traces of the development of a tail were also perceptible. The rapidity of this series of changes is very unusual, if not unexampled.

The comet will doubtless become visible to the naked eye, and will prove an interesting object, although it cannot at present be confidently expected to rival the fine comets of recent years in apparent dimensions and brilliancy.

— Nordenskiold has returned from his exploration of the interior of Greenland, without fully effecting his purpose. From the contradictory reports that have been published by the daily press, we gather that he entered the interior from Auleitsivik Bay, near Disco Island, and himself penetrated to the distance of nearly ninety miles, when the snow became too soft for sledges. His Laplanders pushed much farther on snow-shoes, or about half way across the continent, if they took a direct easterly course, of which we are not assured. On the east coast his vessel subsequently pushed as far northward as Cape Dan, but was prevented from making its way farther northward by the ice.

— The first of the authoritative publications of the International fisheries exhibition contains an excellent account, by G. Brown Goode, of the fishery industries of the United States, both historical and statistical, including all the marine products that are derived from the animal and vegetable life of the seas, as well as a careful though condensed account of the labors of the federal fish commission.

— Professor Simon Newcomb, U. S. Navy, superintendent of the *American ephemeris* and *Nautical almanac*, Washington, and Dr. Benjamin Apthorp Gould, director of the National observatory at Cordoba, Argentine Republic, have been elected corresponding members of the Berlin *Akademie der wissenschaften*.

— According to *Nature*, the balloon of the Paris observatory has been in working order for some weeks. Its capacity being only sixty cubic metres, it was found difficult to use it, except in calm weather. The motions of the registering apparatus are an obstacle to correct readings. The experiments, conducted by Admiral Mouchez, are stated to be only preliminary to further aerostatical experiments. The subject is quite new, scientific ballooning being only in its infancy; and it is only by gradual investigation that the extent of the services it can render to science can be ascertained.

— Professor P. Denza discusses in the *Comptes rendus* the question of the connection between eclipses and terrestrial magnetism. From the time of the total solar eclipse of Dec. 22, 1870, regular observations of magnetic declination have been made at the observatory of Moncalieri during the progress of all eclipses of the sun, as well as some eclipses of the moon. The needle has been observed at intervals of only a few minutes on such occasions; and the entire series of observations extends through twenty eclipses, the last being the Egyptian solar eclipse of May 17, 1882. His discussion indicates no connection between the amount of magnetic disturbance and the magnitude of solar eclipses; and in general it may be regarded as established from his investigation, that the passage of the moon between the earth and the sun in eclipses of the latter, and the passage of the moon through the shadow of the earth in eclipses of the former, have no influence whatever upon terrestrial magnetism.

— The *Illustriste zeitung* reports that the fossil remains of several iguanodons have been found at Bernipart, in Belgium. The skeleton of one of these fossil monsters has been carefully put together, and removed to the Natural history museum at Brussels, where a special case has been made for it, and placed in the courtyard, no convenient space being found inside. The same journal reports the discovery of the remains of animals of the bronze age, made during the extension of the fortifications of Spandau. Among other things were the bones of a species of dog, the leg-bone of a gigantic horse, and the bones of a small species of pig, somewhat like the present Indian one. The remains have been examined by Professor Nehring, who also discovered the remains of a small-limbed goat and of a sheep.

— Mr. Winslow Upton, of the U. S. signal office at Washington, has been elected professor of astronomy at Brown university, Providence, R.I. It is understood that his acceptance of the position is conditional upon the erection of an astronomical observatory which the college authorities have under consideration.

— Professor Piazzi Smyth has published his views upon the subject of a prime meridian for the whole world. They furnish an excellent illustration of the fact that a man's peculiar opinions on any one subject may warp his judgment upon matters wholly removed from it. He advocates the adoption of the meridian of the Great pyramid, because it "passes over solid, habitable, and for ages inhabited, land through nearly the whole of its course from north to south. Its line is capable, therefore, of being laid out along almost all that distance by trigonometrical measurement, and marked by masonried station-signals." Among other equally cogent arguments are the statements that the pyramid "dates from before all human written history, all known architecture, all living architecture;" that "its meridian divides the lands and numbers of the people of the earth much more nearly than any other;" and that it passes not very far from Jerusalem, near which the prime meridian of the world ought to be located by Christian people.

The last idea is developed more fully by M. du Caillaud, who has addressed a letter to the president of the Paris geographical society, urging the adoption of the meridian of Bethlehem, thus harmonizing the longitude reckoning with the customary method of numbering the years from the birth of Christ.

— At Wabash college, Crawfordsville, Ind., a new laboratory is in process of erection, which is to be devoted entirely to biological work. One room, 50×100 feet, with balcony and side-aisles, will contain the general collection of many thousand specimens; a second room will contain the herbarium of twenty thousand species; and a third will be devoted to other collections. Special students are directed to the fact that the collection of crinoids from the Keokuk

beds in the vicinity is complete, and that botanical material is on hand in great abundance for consultation. There will be three laboratories provided with every needed appliance, — one for general botanical work, the second for zoölogical work, the third for special work with compound microscopes. The last laboratory, in particular, is to be devoted exclusively to original research.

— Frederick A. Fernald, in criticising, in the September *Century*, Mr. A. Melville Bell's paper in Science for June 1, objects to the forms of the visible-speech letters which Mr. Bell would employ as symbols for the six consonant sounds in our language which have no proper letters to represent them, and suggests the discarding from their present use in our alphabet of the duplicated symbols *q*, *x*, and *c*, and using them instead for the sounds represented by *ng*, *zh*, and *ch*. "Perhaps it will be decided to replace *w* and *y* by vowels, as in Franklin's scheme; if so, these, with one Anglo-Saxon letter, alredy lookt upon with favor, would make up the six lacking consonants." To this suggestion it may be objected, that the use of familiar letters in an unfamiliar sense would be a source of constant confusion. For example, we should have to read cat and coke as *chat* and *choke*, pleaxure as *pleasure*, roux as *rouge*, sig as *sing*, etc. The alterations of spelling, too, would be seriously numerous; as in siks for *six*, egzist for *exist*, kueen for *queen*, kuite for *quite*, etc. The use of the Anglo-Saxon Þ and ð for the two sounds of *th* would certainly be an improvement on present practice; but the writing of *w* (in *way*) and *y* (in *yea*) as vowels would be altogether wrong, as these sounds are demonstrably not vowels, but consonants. *Wh*, also, is a true consonant, — the non-vocal correspondent of *w*, — and has not, as alleged, the sound of *hoo*. If *wh* had this sound, the sentence, "I saw the man whet the knife," would not be — as it is — unmistakably distinct from "I saw the man who ate the knife." *Ch* (in *chair*) is not, as alleged, a simple consonant, but a compound consisting of a shut position of the tongue (*t*), followed by a hiss (*sh*) ; and either the silent position or the hiss may be prolonged *ad libitum*.

"Even *such* a man, so woe-begone," etc.

Give due lingering emphasis to the word 'such,' in the above quotation, and the compound character of the *ch* — misunderstood by many writers — will be apparent. The letter c should consistently stand for *sh*, not *ch*, in Mr. Fernald's proposition; but, if a better method of completing our alphabet cannot be adopted, by all means rather let the A B C remain as it is.

— *Le Temps* has published the following directions by Pasteur to those exposed to the contagion of cholera.

The precautions to be taken, indicated to the members of the French cholera commission, all relate to the case when it is necessary to guard against the excessive causes of contagion.

1°. Do not use the potable water of the locality, when the commission enters on its investigations, without having first boiled the water, and, after it has cooled, shaken it for some minutes (two or three minutes are sufficient) in a bottle half full and corked.

One may use the waters of the locality, provided one draws them at a spring, in vessels which have been purified by exposing them to a temperature of 150° C., or, better, to a higher heat. One can advantageously employ natural mineral waters.

2°. Use wine which has been heated in bottles some 50° to 60°, and drink from glasses likewise purified.

3°. Only make use of food thoroughly boiled, or of fruits well washed with water which has been boiled, and which has been kept in the same vessels in which it was boiled, or which has been transferred from these vessels to others disinfected by heat.

4°. The bread used should be cut in thin slices, and kept at a temperature of 150° C. for twenty minutes or more.

5°. All vessels used for food should be exposed to a temperature of 150° C. or more.

6°. Bed linen and towels should be plunged in boiling water, and dried.

7°. Water for washing should be boiled, and have added to it, after cooling, one five-hundredth part of thymic acid (one litre of dilute alcohol for two grams of acid) and one-fiftieth part of phenic acid (one litre of water for twenty grams of acid).

8°. Wash the hands and body often during the day with the boiled water to which the thymic or phenic acid has been added.

9°. It is only in case one has to handle the bodies of those who have died from cholera, or the clothes and linen soiled with their discharges, that it is necessary to cover the mouth and nostrils with a mask, formed of two pieces of fine wire gauze, with wadding between, one centimetre thick. The mask should be exposed to a temperature of 150° each time before it is used.

— The Wisconsin agricultural experiment-station was organized by the board of regents of the University of Wisconsin, in June, 1883. The work of the station is in charge of W. A. Henry, agriculture ; William Trelease, botany and horticulture; H. P. Armsby, agricultural chemistry. The bulletins of the station will be sent to all interested. The first number contains an account of experiments at the station in feeding skim-milk to calves and pigs.

— Mr. C. F. Mabery has resigned his position at Harvard college, and accepted the chair of chemistry at the recently organized Case school of applied science at Cleveland, O.

— The British association for the advancement of science will meet next year in Montreal, on Aug. 27.

— Next year's meeting of the Swiss naturalists will be held at Lucerne.

— The jury which will examine the electric lighting machinery offered for competition at the Cincinnati industrial exposition has begun its work. It is hoped that the comparative tests will be the most satisfactory yet obtained. Awards of five hundred and three hundred dollars will be made for the best

and second best systems of electric lighting, both incandescent and arc.

— We learn from *Symons' meteorological magazine* for August, that the government has granted Professor Lemström a sum of 37,000 marks for the continuation of his auroral experiments in Finnish Lapland. These investigations will include the electrical current which produces the aurora, terrestrial currents, and magnetic perturbations.

— Professor Henry F. Osborn of Princeton has published in the July number of the *Quarterly journal of microscopical science*, in a more extended form and with the accompaniment of a lithographic plate, the results of his researches on the foetal envelopes of marsupials. The interest and importance of these investigations are already known, at least to the embryologists among our readers, for Professor Osborn's conclusions were first published in SCIENCE.

— The thirty-eighth volume of the *Mémoires du dépôt de la guerre*, recently printed by the Russian general staff under the editorship of Rylke, and of which only one hundred and fifty copies are issued, contains, among other things, an account of the astronomical and trigonometrical work in eastern Siberia, by Bolsheff, Polianoffski, and Kramereff; a memoir of Kulberg on the Russian geodetic operations in Armenia; a report by Lebedeff on triangulation, topographic and astronomical work in Bulgaria; a list of astronomical stations in the Khirgiz steppes by Bondorf; and Stebnitzki's report on the results of his experiments with a reversible pendulum. Of this important work, absolutely necessary for those seriously interested in the study of the geography of Russia and adjacent countries, there is probably not a copy in America.

— Mr. Miles Rock, assistant astronomer at the U.S. naval observatory, has accepted the appointment of chief astronomer and engineer commissioner on the international boundary commission of Guatemala, to locate the boundary between that country and Mexico. He will sail from New York Oct. 1, and expects to be absent about a year.

— Professor H. M. Paul, late of the Imperial university of Tokio, has returned to this country, and accepted a position in Washington under the Transit of Venus commission.

— The *Comptes rendus* of Aug. 13 gives some extracts from a letter of M. A. Richard to M. de Lesseps, on the cultivation of date-palms. M. Richard states that these palms grow best on a soil saturated with salt, as has been proved at Alicant and elsewhere. The land around Elche, in Valencia, is irrigated from the Vinalopo, which is extremely salt, rising, as it does, in Mount Pinoso, the rocks of which contain much salt and sulphate of lime. This water, having been used for centuries for watering the palm-plantations, has at last formed a crust, which has to be broken with a pick-axe to admit the water below. The town of Alicant has planted its beautiful boulevard along the shore with date-palms, and, as fresh water is very precious there, the trees are regularly watered with sea-water. All the plantations recently made along the shore from Huertas to Rio Monegro have their roots literally in the sea-water, being planted at but a few feet from the sea.

RECENT BOOKS AND PAMPHLETS.

Baillon, H. Traité de botanique médicale phanérogamique fasc. i. Paris, *Hachette*. (To be completed in two fascicules.) 2,301 fig. 8°.

Belfield, W. T. On the relations of micro-organisms to disease. Chicago, 1883. 12°.

Bergstedt, N. H. Bornholms flora. del i. Phanerogamae. Nexoe, 1883. 8°.

Berlin. Königliche museum. Amerika's nord-west-küste: neueste ergebnisse ethnologischen reise. (Ed. by Bastian.) Berlin, *Asher*, 1883. 4+13(+13) p., 13 pl. f°.

Bert, P. Histoire naturelle. Anatomie et physiologie animales. Paris, *Masson*, 1883. 8+382 p., 270 fig. 18°.

Black, William George. Folk-medicine; a chapter in the history of culture. London, *Stock*, 1883. (Publ. Folk-lore soc. 12.) (10)+228 p. 8°.

Borzì, A. Studi algologici. Saggio di ricerche sulla biologia delle alghe. fasc. i. Chlorophyceae. Messina, 1883. 119 p., illustr. 4°.

Buffalo microscopical club. Eighth annual meeting. Secretary's annual report; president's annual address; list of officers and members. Buffalo, *Baker, Jones, & Co., pr.*, 1883. 2+17 p. 8°.

Chabaud, N. Des accidents observés dans les appareils à air comprimé employés aux travaux sous-marins et particulièrement de ceux dus à une décompression trop brusque. Paris, *impr., Davy*, 1883. 55 p. 8°.

Chalon, J. Résumé de cosmographie. Verviers, *impr. Gilon*, 1883. 128 p., illustr. 12°.

Cocconi, G. Flora della provincia di Bologna. Bologna, 1883. 602 p. 16°.

Coleopterorum novitates, recueil spécialement consacré à l'étude des coléoptères. tome i., livr. i. Rennes, *impr. Oberthür*, 1883. 32 p. 8°.

Crié, L. Nouveaux éléments de botanique, contenant l'organographie, l'anatomie, la morphologie, la physiologie, la botanique rurale et des notions de géographie botanique et de botanique fossile. Paris, 1883. 1160 p., 1,332 fig. 12°.

Dejernon, R. Les vignes et les vins de l'Algérie. tome i. Toulouse, 1883. 319 p. 8°.

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